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From the Author

ADDRESS

DELIVERED AT

THE ANNIVERSARY MEETING

OF THE

GEOLOGICAL SOCIETY OF LONDON,

On the 21st of February, 1873;

PREFACED BY

THE ANNOUNCEMENT OF THE AWARD

OF

THE WOLLASTON MEDAL,

THE PROCEEDS OF THE DONATION-FUND,

AND THE MURCHISON MEDAL

AND GEOLOGICAL FUND

FOR THE SAME YEAR.

By THE DUKE OF ARGYLL, K.T., D.C.L., F.R.S.,

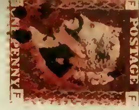
PRESIDENT OF THE SOCIETY.

LONDON:

PRINTED BY TAYLOR AND FRANCIS,

RED LION COURT, FLEET STREET.

1873.



J. F. Campbell Esq. F.G.S.
Niddry Lodge,
Kensington.
W.

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PROCEEDINGS

AT THE

ANNUAL GENERAL MEETING,

21st FEBRUARY, 1873.

Award of the Wollaston Medal.

The Reports of the Council and of the Library and Museum Committee having been read, the President, His Grace the Duke of Argyll, K.T., F.R.S., presented the Wollaston Medal to Sir Philip de Malpas Grey Egerton, Bart., F.R.S., F.G.S., addressing him as follows:—

Sir Philip Egerton,

I consider myself fortunate in being the organ of the Geological Society in presenting you with the Wollaston Medal, which has been awarded to you by the Council for the present year. The eminent services which you have rendered to geology during a period now extending over forty years have long been familiar to scientific men, and have given you more than a European reputation. These services have been so great and so universally recognized, that the only difficulty I now have is not in assigning grounds for the vote which I have the pleasure of announcing, but in explaining why it has been so long delayed. That delay has been occasioned, I believe, solely by the fact that you have yourself been so long an honoured member of the Council whose duty it is to consider the claims of geologists for the honours of this Society; and whatever influence you have had in that body has doubtless been exerted in favour of others to the exclusion of yourself. It is at least some compensation for the loss which the Council sustains in your absence that it is now able to accord a recognition which has long been due. The many papers which you have contributed to this Society from 1833 down to the present time are a sufficient indication of the wide range of your observations. But the special attention you have bestowed, and the light you have thrown on the structure and affinities of fossil fishes and reptiles, have been of the highest value, and have

formed in the aggregate a most important contribution to our knowledge of the history of organic life. I have the highest pleasure in now handing to you the Wollaston Medal.

Sir Philip Egerton, in reply, said:—

My Lord President,

I know not whether it is owing to the poverty of the English language or to my unskilfulness in the use of it; but I am quite at a loss for words adequate to express my appreciation of the great and unexpected honour conferred upon me by the award of the Wollaston Medal, and for appropriate terms to convey to your Grace my acknowledgments for the kind but too flattering terms you have used in communicating the decision of the Council; and my embarrassment is increased by the consciousness that, in comparison with those illustrious names which already adorn the Wollaston roll, I am quite unworthy of this great distinction. I cannot presume to think that the humble contributions I have been enabled to make to geological knowledge (and indeed to but a limited branch of it) can have been weighed in the balance against the labours of many others on both sides of the Atlantic, whose lives have been devoted to geological research, but who have not yet attained the distinction awarded to me to-day. In comparison with these my claims are quite insignificant. I must therefore look elsewhere to discern the motive which has influenced the Council in selecting my name on the present occasion in preference to others whose scientific claims are far greater than my own; and I think I am right in assigning it to a desire on their part to recognize, encourage, and occasionally reward the labours of those who, although their lot in life has been cast in a sphere entailing many paramount duties which ought not to be neglected, nevertheless devote their leisure time to the promotion of scientific research rather than waste it in frivolous and unproductive amusements. In this sense I interpret the mind of the Council in awarding me this Medal; and in this sense, as also as a stimulus and incentive to persevere in the cause of that science in which I take so deep an interest, and from the study of which I have derived so much intellectual enjoyment, I can, without arrogance, most gratefully accept it. May I be permitted to add that if any thing could enhance the feelings of gratification I experience in receiving this, the *blue ribbon* of geology, it is that it is presented by a President who, although occupying the

highest social rank, and called by our gracious Sovereign to fill the highest offices of State, entailing most onerous duties and grave responsibilities, has nevertheless devoted himself to the study of scientific problems, and has inscribed for himself a name on the tablets of scientific literature, indelible so long as the Reign of Law shall continue to exist.

Award of the Wollaston Donation-fund.

The President then presented the balance of the proceeds of the Wollaston Donation-fund to J. W. Judd, Esq., F.G.S., and addressed him as follows:—

Mr. Judd,

I have much pleasure in delivering to you the award of the Council of this Society in recognition of your valuable researches in the Neocomian and Jurassic rocks of England, researches which you are now extending with such marked success to the Secondary and Palæozoic rocks of Scotland. I rejoice to know that you are to carry to an investigation of the West coast of Scotland the experience and knowledge you have shown in your recent account of the Secondary rocks of the East coast. The scattered and broken remains of the Oolites in the Hebrides constitute a most interesting field of investigation; and a detailed examination of them conducted by you cannot fail to cast important light on many geological problems of the highest interest to our science.

Mr. Judd made the following reply:—

My Lord President,

The recollection of an occasion like the present may well be cherished by a student of science as an incentive to exertion second only to the enthusiasm of research itself. Having learned to look to this Society, and never in vain, for the encouragement of sympathy and the guidance of criticism, it is with especial gratification that I receive this mark of confidence at the hands of my teachers and fellow workers. When I think of the origin and traditions of this bequest, the objects contemplated by its illustrious founder, the distinguished geologists who have been its former recipients, and the important researches to which it has been made contributory, I am deeply impressed by the trust which you have reposed in me. It is

my hope that by earnest labour I may be able to testify that my feelings of gratitude are not evanescent, nor my sense of responsibility light, in connexion with the great honour which you have this day done me.

Award of the Murchison Medal and Geological Fund.

The President then presented the Murchison Medal to Mr. William Davies, of the British Museum, and addressed him as follows:—

Mr. Davies,

I have much pleasure in delivering to you the Murchison Medal, which has been awarded to you by the Council of this Society in recognition of the services you have rendered to Palæontology in the skill and knowledge you have displayed in the reconstruction of extinct forms of life. I have the more pleasure in giving this Medal, as I believe you will have the greater pleasure in receiving it, from the fact that it is the first award made under and in fulfilment of the Will of the great geologist and excellent man whose loss we have all had so lately to deplore. I trust it may long serve to stimulate others to such services as you have rendered, and which have appeared to the Council of this Society to make you a worthy recipient of the First Murchison Medal.

Mr. Davies in reply said:—

My Lord Duke,

I desire to return my most sincere thanks to your Grace as President, and to the Council of this Society, for the honour they have conferred upon me in awarding me the Murchison Medal. It is extremely gratifying to find that the humble services I have rendered to Palæontological science have been so kindly appreciated and deemed worthy of this high recognition. The pleasure is greatly enhanced by the fact that I have never considered my scientific work of sufficient importance to deserve any recognition—the acquisition of scientific knowledge and the happiness of communicating it to others having, in my own case, been an ample reward. I shall now feel it to be my duty as well as my ambition to render myself more worthy of the distinction you have this day conferred upon me—one

which has also an especial significance to a servant of that great National Institution for which Sir Roderick Murchison so long and beneficially acted as a Trustee.

The President then delivered to Prof. ANSTED, F.R.S., For. Sec., for transmission to Prof. OSWALD HEER, of Zürich, the balance of the Murchison Geological Fund, and spoke as follows:—

Mr. SECRETARY,

The labours of Prof. Heer in Fossil Botany and Entomology have this year been recognized by this Council in the vote of the Murchison Fund. No branch of Palæontology requires more minute research, more careful comparison, more circumspect conclusions; and there are none, I may add, which, when so conducted, are richer in suggestions on the history of geological change. The fragmentary character which generally belongs to terrestrial and especially to botanical remains, places the study of them under special difficulties—difficulties which have been met with special skill by Prof. Heer. The remains of the Miocene flora are connected with some of the most perplexing problems of our science; and the light which has been thrown upon them by Prof. Heer more than deserves the recognition which I have now the pleasure of delivering into your hands for transmission to that distinguished man. This is the second mark of recognition which this Society has given to Prof. Heer, the Wollaston Donation Fund having been voted to him in 1862.

Prof. ANSTED having suggested that Sir Charles Lyell, as a particular friend of Prof. Heer's, might very appropriately speak in his name, Sir CHARLES LYELL in reply referred briefly to the nature of Prof. Heer's work, and said that he was sure that gentleman would appreciate highly this renewed expression of the interest taken by the Geological Society in his pursuits. Sir Charles Lyell remarked, further, that he was particularly gratified that this award had been made at the present time, as Prof. Heer was now able to resume his labours after the infirm state of his health had of late given his friends much anxiety.

THE ANNIVERSARY ADDRESS OF THE PRESIDENT,
His Grace the Duke of Argyll, K.T., F.R.S.

Gentlemen,—Before commencing the reading of my Address it is my duty, in the first place, to mention some of the more noted of the Fellows, intelligence of whose decease has been received since the last Anniversary Meeting*.

Of the twenty-one Fellows of the Society whose names have in this way been removed from our List, there are very few who have contributed any thing to the scientific Proceedings of this Society, or, indeed, in any public fashion to the progress of geology. There is, however, one of those whose loss we have to deplore, whose name stands at the very head of the list of students of our science, and who was one of the veteran band of geologists by whom the very foundations of that science were laid. Last year your President had to refer to the death of Sir Roderick Murchison; to-day I have to mention the decease of one with whose name that of Murchison will always be associated.

Adam Sedgwick was born on the 22nd of March, 1785, at Dent, in Yorkshire, where his family had been settled for very many years. His father was the vicar of the parish, a man of great local influence and of considerable learning. The young Sedgwick was educated at Sedbergh school, and went thence to Cambridge, where he entered Trinity College in 1804. At Cambridge he read chiefly mathematics, and returning to Dent during the vacations continued his studies with the self-taught mathematician Matthew Dawson. In 1808 he took his degree, when he was classed as 5th Wrangler; and in the following year he was elected to a Fellowship in Trinity College, of which he subsequently became Assistant Tutor. He then threw himself energetically into the ordinary work of the University, until in 1818 he was appointed Woodwardian Professor of Geology in the room of Prof. Hailstone. In the same year he was elected a Fellow of the Royal Society and of this Society, and was ordained priest, having taken deacon's orders in 1817. In 1819, in conjunction with others, he was mainly instrumental in founding the Cambridge Philosophical Society, of which he was one of the first secretaries, his colleague in that office being Professor Lee.

The formal duties of the Woodwardian Professors were of rather

* As the pressure of public affairs prevented the President from writing the Obituary Notices, they have been prepared for him by the Assistant-Secretary. Mr. Seeley, F.G.S., kindly furnished important assistance in the preparation of the memoir of Professor Sedgwick.

a singular nature. According to the terms of the founder's will they were appointed specially for the purpose of defending Dr. Woodward's views as to the nature and origin of fossils against the attacks of Dr. Camerarius, of Tübingen, and his disciples or followers, an engagement to this effect being entered into by each Professor on his being appointed to the chair. This engagement was always faithfully fulfilled by Prof. Sedgwick in his inaugural lecture of each year. Besides this duty the Professor had the charge of the Woodwardian Collections, and was also required to correspond, when necessary, on scientific matters with distinguished foreigners. The object of this correspondence was the procuring of fresh specimens for the museum; and for defraying its expense, and the direct purchase of specimens, a sum of £10 annually had been specially left by Dr. Woodward. The geological heresies of Dr. Camerarius probably gave but little uneasiness to the Woodwardian Professors, who also thought that the £10 a year allotted to defray the expense of foreign correspondence and the purchase of specimens might be better employed, so that prior to Sedgwick's election the Professorship seems to have become almost a sinecure, and scarcely any of his predecessors had given lectures on geology.

On Sedgwick's appointment, however, it was made a condition that he should lecture. His knowledge of geology at this time was probably of the most scanty description (he himself said, later in life, that he had merely observed some phenomena of stratification in the Carboniferous Limestone about Dent); and his appointment, with the condition attached to it, must have been due to a conviction on the part of the University authorities that a high Wrangler must be able to do whatever man could do, rather than to the recognition of any special fitness of the newly appointed Professor for the functions assigned to him. But whatever may have been his deficiencies in this respect, he set himself most energetically to supply them. Almost immediately after his appointment he started in company with Prof. Henslow to investigate the geology of the Isle of Wight; and upon the materials collected in this excursion his first course of lectures was founded. From this time he allowed no opportunity of acquiring geological knowledge to escape him; and as early as May 1820 we find him communicating to the Cambridge Philosophical Society his first paper, in which he treats of the physical structure of Devonshire and Cornwall. Early in his scientific career he visited Paris in successive winters, where he benefited by the instructions, and enjoyed the friendship, of Cuvier and De Blainville, and of other eminent men who were then at the head of science in France.

In 1829 Professor Sedgwick was elected President of this Society; and he served that office for the usual term of two years. Both before and after this date he communicated many papers to the society, several of them prepared in conjunction with the late Sir Roderick Murchison, his intimacy with whom commenced at the very outset of the latter's scientific career. In company with Murchison he visited many parts of Britain and the continent of Europe; and the results of their travels are to be found in their joint papers.

In 1833 Sedgwick was President of the British Association during its first meeting at Cambridge.

In 1834 he was presented to a Canonry in Norwich Cathedral, by Lord Brougham, who was at that time Lord Chancellor. He was subsequently offered various pieces of church preferment, but always declined them, as their acceptance would have severed his connexion with the University.

In February 1851 the Council of this Society awarded to Prof. Sedgwick the Wollaston Palladium Medal, the highest honour in their power to bestow, in recognition of the value of his "original researches in developing the geological structure of the British Isles, the Alps, and the Rhenish Provinces." Twelve years later, in 1863, he received from the Royal Society the Copley Gold Medal "for his observations and discoveries in the geology of the Palæozoic series of Rocks, and more especially for his determination of the characters of the Devonian system, by observations of the order and superposition of the Killas rocks and their fossils in Devonshire."

When responding to the speech of the President and expressing his thanks to the Council of this Society for the award of the Wollaston Medal, Prof. Sedgwick spoke of himself as nearly approaching "that limit of man's age, beyond which his days are 'but labour and sorrow,'" and as having probably but few more years to devote to the advancement of the science to which he had done such good service. His health, in fact, had never been very robust; and for years before the date referred to he may be said to have been an habitual invalid, compelled to pay the strictest attention to his diet in order to avoid serious illness. And subsequently to this date his writings on geological subjects were but few. For many years he constantly referred to his sense of the increasing infirmities of age, and declared from year to year that they would compel him to resign his Professorship. Nevertheless, being convinced by his friends that his resignation of that position would be injurious to the cause of science in the University of Cambridge, he continued to occupy the chair until his death, which took place on the 27th of January in the pre-

sent year, in his rooms in Trinity College, of which he had long been the Senior Fellow. At his death he was within six weeks of completing his 88th year. On the 1st of February his remains were consigned to the tomb in the ante-chapel of Trinity College, and he was followed to his last resting place by most of the resident members of the University and by many distinguished scientific men anxious to pay this last tribute of respect and affection to one who had laboured so effectually for the advancement of science, and whose personal character had endeared him to all who knew him.

In estimating the influence exerted by Professor Sedgwick during his long life upon geological science, we have to consider him in various lights,—in his connexion with the University as a teacher of science, and as advocating the claims of science to recognition as a legitimate branch of culture in opposition to those vehement prejudices which existed, especially against geology, in the minds of many leading men—in the effect of his personal character upon the minds of those with whom he was brought into direct contact—in his vigorous original investigation of geological facts, and his writings on geology—and in the untiring energy and noble liberality which he displayed in the acquisition and arrangement of the magnificent collections now contained in the Woodwardian Museum.

In the first of these capacities, namely as a teacher of geology, his lectures, till within the last ten years of his life, were characterized by a philosophical grasp of the principles on which geology as a science must rest, equal prominence being given to the physical and palæontological evidences. His delivery was most eloquent, and often impetuous, his style simple and logical; in the construction of his lectures there was a unity of plan which rendered it easy for his hearers to carry away with them the clearest notion of the subject of which he had been treating; and the current of his discourse was always diversified by strokes of humour which served to enliven his treatment even of the driest subjects. This humour, and the enthusiasm which Prof. Sedgwick always displayed, and which fairly carried his audiences away with him, constituted the great charm of his public discourses.

In the more intimate relations of life the same qualities also gave him immense influence over those with whom he associated; and as his students were freely admitted to intercourse with him, and indeed even induced by every means to seek his society, both in the cabinet and in the field, it can be no matter of wonder that so many

active workers in geology, among whom may be mentioned such names as those of Darwin, Jukes, and Ansted, were inspired by the precept and example of Sedgwick to labour in that field which he cultivated with such enthusiasm and success.

At the time of Sedgwick's appointment as Woodwardian Professor, the science of geology was in its infancy, and amongst the more narrow-minded of the orthodox party in Cambridge there must have been many who regarded it with alarm, and would have rejoiced to strangle it in its cradle. Under these circumstances no better selection could have been made than that of Prof. Sedgwick, whose extraordinary mental energy peculiarly fitted him for maintaining what he believed to be the truth against all opposition. And throughout the course of his University life he always consistently advocated the recognition of natural science as a legitimate subject of academic study, and its full right to obtain for its votaries honours equal to those assigned to successful students in mathematics and classics. Probably the opposition he met with in his efforts towards this end, coupled perhaps with recollections of a time when his geological faith was almost a matter of opprobrium, may have urged him to take up the cause of other reforms in the University, and to advocate strongly the breaking down of all those religious and intellectual tests which sought to confine the advantages of University training to those who held by one particular way of thinking. At the same time it must be admitted that the course he took in connexion with these reforms was in accordance with the whole tenor of his life, and especially with that strong sense of justice which was one of his most prominent characteristics.

As a geological investigator and writer the chief object which he had set before him was, as he himself used to say, "the unravelling of the story of the older Palæozoic rocks." His first essays in this direction were made at a very early period of his career, when he investigated the disturbed strata of the Killas and associated rocks in the counties of Devon and Cornwall. His first paper, already alluded to as communicated to the Cambridge Philosophical Society in 1820, was upon these formations; and as he was no doubt attracted to their investigation by the mathematical tendencies of his mind, due to his Cambridge education, and his subsequent work evidently took its character from this commencement, we may regard the particular direction in which his chief studies were pursued as due in the first instance to his mathematical training. To this also, espe-

cially to his thorough mastery of solid geometry, much of his brilliant success as a physical geologist is due.

The south-western portion of England continued to occupy Prof. Sedgwick's attention for many years; and he published several papers upon its geology, some of them prepared in conjunction with the late Sir Roderick Murchison. The investigations of these two friends, the final results of which were embodied in a joint paper on the physical structure and older stratified deposits of Devonshire, led, through the palæontological researches of Lonsdale, to the establishment of the Devonian system.

Between 1822 and 1828 Prof. Sedgwick devoted considerable attention to the Magnesian Limestone of the north of England, and described the results of his investigations in papers communicated to this Society and published in the third volume of the second series of our Transactions. In these papers he described the characters and traced the extension of this formation, which had previously been indicated by Smith, in Yorkshire, under the name of the "Pontefract Rock." He first noticed the peculiar concretionary structure which characterizes some portions of the formation, established its subdivisions very nearly in the manner at present adopted, and indicated their correspondence with foreign equivalents. This description of the Magnesian Limestone must be regarded as the most complete production of Prof. Sedgwick's pen.

It was probably the observation of the remarkable forms presented by the concretionary Magnesian Limestone (which evidently produced a great effect on his mind) that induced Prof. Sedgwick to turn his attention soon after the reading of this paper to the causes of such peculiarities, and led to the production of his paper "On the structure of large mineral masses, and especially on the chemical changes produced in the aggregation of stratified rocks during different periods after their deposition," which was read before this Society on the 11th of March, 1835, and published in the third volume of the second series of our Transactions. It contains an excellent discussion of the phenomena of metamorphism, concretionary structure, cleavage, and joints; the terms "cleavage" and "strike" are here first introduced into geology.

In 1822–24 Sedgwick was engaged in investigating one of the most difficult problems in British geology, namely the structure of the Cumbrian mountains. His first paper upon this subject was communicated to this Society in January 1831—and was followed by others, terminating on the 2nd of February 1848. In the mean-

while he had been engaged, partly alone, partly in company with Sir Roderick Murchison, in the investigation of the geological structure of Wales, Sedgwick working upwards from those disturbed and obscure rocks, which seem always to have possessed for him a peculiar fascination, while Murchison commenced his labours upon the higher deposits, the comparatively undisturbed state and abundant fossil contents of which seemed to offer a problem more easy of solution. Sedgwick's results, obtained in the first year of his labours and communicated to the British Association in 1832, and to the Cambridge Philosophical Society in March 1833, seemed to him to furnish a clue to the whole structure of the country; for he states that in Caernarvonshire and Merionethshire "the strata are bent into *saddles* and *troughs*, of which the *anticlinal* and *synclinal* lines occur alternately, and are all nearly parallel to the 'great Merionethshire anticlinal line.'" It was by sections taken across these chief anticlinal and synclinal lines that Sedgwick was enabled to work out the stratigraphical succession, and to characterize the principal subdivisions of what in 1835 he denominated the Cambrian system. At the same time Murchison had reached what he regarded as the base of his Silurian system; and in the autumn of 1835 the two investigators communicated to the Meeting of the British Association in Dublin a general account of the combined results of their several labours, &c., by which it was supposed that the Cambrian and Silurian systems were definitively established as distinct and successive series of formations. It was soon found, however, that the actual boundary between the two systems had not been accurately ascertained; and by degrees a controversy arose upon this point, which cannot yet be regarded as absolutely settled. To the last Sedgwick maintained his original views, which were reiterated in almost the same form in his later writings, the most important of which was the "Synopsis of the Classification of the British Palæozoic Rocks," prefixed to Prof. M'Coy's descriptions of British Palæozoic Fossils.

Of Prof. Sedgwick's numerous other contributions to geological literature, most of which related to the Palæozoic series of rocks, only a few can be here briefly referred to. At an early period of his acquaintance with Murchison the two friends devoted themselves to an investigation of the Isle of Arran, the results of which are given by them in an important paper published in our Transactions. A little later Sedgwick and Murchison examined the structure and relations of the Palæozoic rocks of the north of Scotland; and from this investigation originated their valuable joint paper on this

subject, also published in the Transactions of the Society. The results of various Continental excursions were also communicated to this Society; in these papers the authors discussed, especially, the structure of the Eastern Alps and of the North of Germany and Belgium, their final contribution to this series being the valuable memoir "On the Distribution and Classification of the Older or Palæozoic Deposits of the North of Germany and Belgium, and their comparison with Formations of the same age in the British Isles," published in the sixth volume of the second series of our Transactions.

Prof. Sedgwick contributed but little to the literature of Secondary Geology. Nevertheless one of his earliest investigations was that of the Secondary strata of the Yorkshire coast, which he examined in 1821, although the results of his researches were not published until 1826, when he found that Young and Bird's 'Geology of the Yorkshire Coast' did not by any means come up to his expectations. In his paper Sedgwick endeavoured to make up for the deficiencies of this book by a correlation of the beds observed by him with those occurring in other parts of England. Twenty years later, in 1845, Sedgwick communicated to the Meeting of the British Association in Cambridge a paper on the geology of the neighbourhood of that city, in which he clearly indicates the outlines of nearly every thing of importance that has since been done in the investigation of that district.

In all these writings he exhibits the same characteristics—indefatigable energy in the observation of facts, the most penetrating sagacity in grasping their significance, and the broadest possible power of generalization.

There is yet another mode of activity by which Prof. Sedgwick has exerted a most profound influence upon the progress of geology, and by which, whilst raising an imperishable monument to his memory, he has provided one of the best securities for the continued successful cultivation of his favourite science in Cambridge. On his receiving the appointment of Woodwardian Professor, the collection of which he was put in charge consisted solely of the original collections of British and foreign Fossils and Minerals brought together in the 17th century by Dr. Woodward. From the first he set himself vigorously to the task of forming a museum worthy of the University; with this view, wherever he went, he collected rock specimens and fossils, and used all his influence with the University authorities to induce hem to make purchases of fine specimens or

collections too costly to be secured at his own expense. Among the most important of these acquisitions may be mentioned the collection of Count Münster's duplicate fossils, the Image Collection of Chalk Fossils, Fletcher's Silurian Collection, and the Leckenby Collection of Oolitic Fossils, chiefly from Yorkshire, the last mentioned magnificent series having been purchased with funds subscribed in response to an appeal made by Prof. Sedgwick, in which he speaks in a most touching manner of his being prevented by the infirmities of age from enriching the collections by his personal efforts.

For many years the great accumulations of specimens of all kinds which were gradually finding their way into the hands of the Cambridge Professor had no fitting place for their reception, the only apartment assigned to him for this purpose being a room under the Divinity Schools, in which the specimens were stored away, generally in the packages in which they had come to Cambridge. It may easily be imagined that such a state of things would be by no means satisfactory to Prof. Sedgwick; and he exerted all his influence to put an end to it. At length, in 1842, he obtained possession of the fine suite of rooms under the new building of the University Library, in which the collections are at present displayed; and now, as might have been expected, he devoted himself with his accustomed enthusiasm to the task of getting the collections arranged in the manner best adapted to advance the study of geology in the University. With this view he secured the assistance of a succession of able coadjutors in the persons of Ansted, Jukes, Salter, M'Coy, Barrett, and Seeley, under whose hands the treasures of the Museum were gradually brought into order, and in many cases described. Thus the fine collection of Palæozoic fossils gave origin to the 'Descriptive Catalogue' of Prof. M'Coy, published in 1851–55, and to the 'Contributions to British Palæontology' of the same author; whilst of late years Mr. Seeley has published a Catalogue of the Reptilia of the Secondary strata, and a descriptive Catalogue of the remains of Ornithosauria from the Cambridge Greensand, founded on the specimens contained in the museum. Mr. Salter had also prepared an Index supplement to Prof. M'Coy's descriptive Catalogue. This was nearly completed at the time of Mr. Salter's death, and has since been edited by Prof. Morris, and furnished by Prof. Sedgwick with an elaborate preface, which will explain fully his views as to the nomenclature and classification of the older Palæozoic rocks, and serve as a summing up of his side of the question at issue between Cambria and Siluria. Near the end of this pre-

face Prof. Sedgwick expresses himself as follows:—"There were three prominent hopes which possessed my heart in the earliest years of my Professorship:—first, that I might be enabled to bring together a collection worthy of the University, and illustrative of all the departments of the science it was my duty to study and to teach; secondly, that a Geological Museum might be built by the University, amply capable of containing its future collections; and, lastly, that I might bring together a class of students who would listen to my teaching, support me by their sympathy, and help me by the labour of their hands. It now makes me happy to say that all these hopes have for many years been amply realized."

Mr. William Bainbridge of Newcastle-on-Tyne, who joined the Society in the year 1846, was the son of Mr. William Bainbridge, Solicitor, of Alston Moor. He was called to the Bar in 1838, and acquired some eminence as a Conveyancer. His only contribution to Geological literature is a paper 'On the Pennine Fault in connexion with the Volcanic Rocks at the foot of Cross Fell, and with the Tynedale Fault, called the ninety-fathom Dyke,' which was communicated to the Geological Section of the British Association during the Meeting at Newcastle in 1863. In connexion with his profession, however, he published 'A Treatise on the Law of Mines and Minerals,' which has passed through three editions. Mr. Bainbridge died last year, at the age of 60.

Mr. Nathaniel Beardmore, a Civil Engineer of considerable eminence, especially in connexion with hydraulic works, was born at Nottingham, in 1816. He became a Fellow of this Society in 1848, but rarely attended its meetings, and never communicated any thing to its publications. An important work connected with his immediate profession, namely his 'Manual of Hydrology,' contains much information of great value to the geologist. The foundation of this book, published in 1850, consisted only of Hydraulic Tables for the use of Engineers in the solution of questions connected with hydraulic operations. This was expanded into the 'Manual of Hydrology,' which appeared in 1862, and contains, besides the Tables, a great amount of valuable information upon tides, rainfall, evaporation, and the flow of rivers, and also upon springs and wells. For many years Mr. Beardmore was a leading authority upon all questions relating to water-supply. He died on 24th August, 1872.

Mr. Thomas Bloxam was born in London, in 1836, and during his education, at the City of London School, showed an early predilection for scientific studies, which led him to discharge the duties of Assistant to the Lecturer on Chemistry and Natural Philosophy. He subsequently studied Chemistry at King's College, where his brother, Prof. Bloxam, was then Demonstrator in Chemistry; and in 1855 he was appointed Chemist to the Industrial Museum of Scotland, a position which he held until 1859. Mr. Bloxam then became Lecturer on Chemistry in the Medical School of St. George's Hospital, where he remained until, in 1862, he proceeded to Cheltenham College as Lecturer in Experimental and Natural Science. He remained at Cheltenham until his death in July last. Mr. Bloxam became a Fellow of the Chemical Society in 1859, and of this Society in 1869. He never contributed any papers to our publications; but to the Royal Society of Edinburgh he communicated, in 1857, an 'Analysis of the Craigleith Sandstone,' and, in 1862, papers 'On the composition of the Building-stones of Craigleith, Binnie, Gifnock and Partick Bridge' and 'On the Composition of the Glassy Surface of some vitrified Forts.'

Robert Daun, M.D., was the eldest son of the Rev. George Daun, minister of Insch, in Aberdeenshire. He was educated at Aberdeen and Edinburgh, and took his degree of M.D. in the latter University, after which he entered the Army as Surgeon, and served in this capacity with the Scots Greys at Waterloo. Dr. Daun was subsequently appointed one of the Inspectors General of Hospitals. He joined this Society in the year 1832, but does not appear to have devoted any special attention to Geology, his principal study being Mathematics, in connexion with which he accumulated a large library. For the last ten years of his life he resided in Edinburgh, where he died in 1871 at the advanced age of 86.

Mr. John Samuel Enys (né Hunt) assumed the name of his mother's family on succeeding to their large landed property in Cornwall. He joined the Geological Society in 1840. His only Geological paper 'On the Granite found near Penryn, and on the mode of working it,' appeared in the 'Philosophical Magazine' for May 1833. He took much interest in Cornish mining-operations, and made many communications to the British Association, the Institution of Civil Engineers, and various provincial societies on the subject of pumping-engines for mining-purposes. Mr. Enys also

took an active part in the proceedings of the Philosophical Institutions in his neighbourhood, and zealously promoted the diffusion of scientific knowledge among the miners. He died in the early part of last year, after a long illness, at Enys, the family seat, near Penryn, in his 75th year.

Mr. Philip Hardwick, an Architect of considerable eminence, was born on the 15th June, 1792, and studied his profession under his father, Mr. Thomas Hardwick, who had been a pupil of Sir William Chambers. In 1818 he began to practise as an Architect; and between that year and 1842 (when his health failed him) he designed some of the best modern buildings in London. He was elected a Member of the Royal Academy in 1839, and in 1850 became Treasurer of that body. He became a Fellow of this Society in 1837, and was also a Fellow of the Royal Society and of the Society of Antiquaries, in the proceedings of which he took much interest, although his professional avocations and the state of his health prevented his being an active scientific worker. After a long and painful illness Mr. Hardwick died at Wimbledon on the 28th of December, 1870.

Mr. William Keene, Government Examiner of Coal-fields in New South Wales, was born at Bath in the year 1798. In 1821 he commenced the study of medicine in London, and soon afterwards became acquainted with Mr. Goldsworthy Gurney, the inventor of the Bude light, whom he assisted in his lectures at the Surrey Institution. In 1827, having abandoned the profession of medicine, Mr. Keene went to the south-west of France for the purpose of introducing certain improvements in steam navigation. He then resided for many years in the Pyrenees, devoting his attention especially to the study of Geology; but the revolution caused him to return to England in 1848. In 1852 he left England for New South Wales, where he was appointed one of the Government Examiners of Coal-fields, and, after the death of Mr. Stutchbury, performed all the functions of Government Geologist. In 1864 Mr. Keene assisted the naturalists of the Austrian Frigate 'Novara' with much information upon the geology of Australia; and in recognition of the services rendered by him on this occasion, he was in the following year elected a Corresponding Member of the Geological Institute of Vienna. Collections of the minerals and fossils of Australia were also sent by him to the great Exhibitions which have taken place in London, Paris, and

elsewhere; the collection sent by him to Paris in 1867 received the gold medal. Mr. Keene became a Fellow of this Society in 1865; and in the same year he communicated to the Society a paper "On the Coal-measures of New South Wales, with *Spirifer*, *Glossopteris*, and *Lepidodendron*." Another paper, "On the examination of Brown Cannel or Petroleum Coal-seams at Colley Creek, Liverpool Plains, N. S. W.," was communicated by Mr. Keene to the Society in the following year; and these seem to be his only geological publications. He died on the 2nd February, 1872.

Dr. E. F. Kelaart, F.L.S., was a native of Ceylon, belonging to a family descended from early Dutch colonists. His father was employed in the military medical department; and by his precepts and example young Kelaart was early led to turn his attention to natural history. In this pursuit he was also encouraged by the late Mr. Henry Marshall, Deputy Inspector-General of Hospitals. Having visited England for the purpose of studying medicine and surgery, and obtained his diploma of M.D., he was appointed, in 1841, a Staff Assistant-Surgeon in her Majesty's Forces, and stationed at Gibraltar, the plants of which place he studied carefully. In 1846 he published the results of these investigations in a small work entitled 'Flora Calpensis, or Contributions to the Botany and Topography of Gibraltar.'

Returning to his native island of Ceylon, Dr. Kelaart occupied himself for several years in the investigation of its natural history, upon which he published several papers. A 'Prodromus Faunæ Zeylanicæ' was also commenced by him; and the first volume and part of the second appeared at Colombo in 1852 and 1854. In 1856 he was commissioned to investigate the natural history of the Pearl-oyster of Ceylon; and his 'Introductory Report' on this subject, containing some valuable observations, was published at Trincomalee in 1857.

Dr. Kelaart became a Fellow of this Society in 1845, but never contributed to its publications. His only recorded geological paper, entitled "Notes on the Geology of Ceylon: Laterite formation, fluviatile deposits of Neuera Ellia," appeared in vol. liv. of the Edinburgh New Philosophical Journal (1853).

Dr. Kelaart died suddenly of disease of the heart, on the 31st of August, 1860, when on his passage to England. He was in his 42nd year.

Mr. John Lawson, who joined the Geological Society in 1865, died on the 7th February of the present year, at the age of 48. Mr. Lawson never published any papers on geological subjects; but as a Civil Engineer, connected especially with questions of water-supply and drainage, he showed a thorough appreciation of the value of geological information.

Mr. Charles Longman had been for ten years a Fellow of this Society; and though he never communicated any papers to it, and was not a frequent attendant at its meetings, he took a warm interest in the progress of geological science, especially in all matters relating to the source and flow of water, and to the antiquity of Man. He was the second son of the late Mr. Longman, of Paternoster Row; and both his surviving brothers are Fellows of this Society. His death, which was very sudden, took place on the 4th of January in the present year.

Sir Thomas Phillips, Bart., M.A., F.R.S., F.S.A., was the son of a wealthy manufacturer in Manchester, where he was born in July 1792. His father soon afterwards retired from business to an estate which he purchased in Worcestershire; there he devoted all his attention to the education of his only son, who was sent first to Rugby, and afterwards to Oxford, where he took his degree of M.A. in 1820. After his father's death in 1818, he devoted himself especially to the study of literary antiquities, and in the course of his long life brought together one of the largest collections of rare books and MSS. ever amassed by a private individual. In 1821 he was created a Baronet; and during the last few years of his life he was one of the Trustees of the British Museum.

Sir Thomas Phillips was elected a Fellow of this Society in 1830, but never contributed any thing to its publications, his only paper at all bearing upon any subject related to geological research being one "On the submersion of a part of Hayling Island in the reign of Edward III.," published in 1832 in the 'Journal of the Royal Geographical Society.' Sir Thomas Phillips died on the 6th of February, 1872.

Mr. Augustus Smith, of Tresco Abbey, Scilly, the eldest son of Mr. John Smith, of Ashlyns, Hertfordshire, was born in 1804, and educated at Harrow and at Christchurch College, Oxford. In 1834 he became lessee of the Scilly Islands, where he rebuilt Tresco Abbey,

and thenceforward made it his home. His residence in Scilly produced a most beneficial effect on the islands: he found the islanders in a pitiable condition; the roads were few and bad; the land was so subdivided amongst a too numerous population, that the holdings scarcely sufficed to support their tenants; and the schools, which were but thinly attended, were of the most unsatisfactory description. By judicious exertions Mr. Smith succeeded in obtaining employment at home or elsewhere for the redundant population, constructed a pier, laid out good roads, and established schools, at which he rendered attendance compulsory by making the rents paid by the parents depend inversely on the presence of their children at school. The beneficial effect of these measures on the prosperity of the people of the Scilly Islands has been most striking. From 1857 to 1865 Mr. Smith represented the borough of Truro in Parliament.

During his stay at Oxford Mr. Smith was a constant attendant at the lectures of the late Dr. Buckland, and from an early period of his residence in Cornwall he was a zealous supporter of the county institutions. Of the Royal Institution of Cornwall he was President for two years. The Chair of the Royal Geological Society of Cornwall was filled by him for six years; and the erection of its excellent Museum at Penzance was in great measure due to his energy and perseverance. His only geological publication is a paper "On the Chalk-flints and fragments of Greensand found on one of the islands of Scilly," which appeared in the Transactions of the Royal Geological Society of Cornwall (vol. vii. p. 343). His other papers are "On the opening of a Kist-vaen at Scilly" and "On the capture of the Golden Oriole in the gardens of Tresco," published in the 'Journal of the Royal Institution of Cornwall' (vol. ii.). Many additions to the ornithology of the British islands made at Scilly and recorded in Yarrell's 'British Birds' were due to the vigilance of Mr. Smith, and of his game-keepers.

Mr. Smith died at Plymouth in July last, and, by his own desire, was buried on the mainland, but in a churchyard visible from the spot he had done so much to improve and adorn.

M. Philippe-Marie-Guillaume van der Maelen, born at Brussels on the 23rd December, 1795, was originally destined by his father to a mercantile career; but after trying this for a time, he gave it up, in order to indulge what seems to have been an innate impulse to the study of geography. In 1827 he published his 'Atlas Universel,' in 400 sheets, and this was followed in succeeding years by

other works of the same nature but of narrower range, all of which met with great success. As early as 1830 M. van der Maelen founded, with a view to the preparation and publication of his works, a building known as the "Etablissement géographique de Bruxelles," which not only accommodated the editorial and operative staff necessary for carrying out the ideas of its founder, but also furnished a resting-place for numerous collections, serving to illustrate the natural productions of various countries; and to such an extent was the importance of these illustrations recognized that the Establishment even sent collectors to Mexico, Australia, and other places, in search of botanical specimens. M. van der Maelen became a Fellow of this Society in 1835. He does not appear to have ever written upon geological subjects; but his maps, especially those of Europe and Belgium, are of great value; and of the latter the "Carte des Concessions houillères de Belgique" and the "Carte des Charbonnages des environs de Charleroi," published by the "Etablissement" in 1862 and 1865, may be regarded as geological works. M. van der Maelen died on 29th May, 1869.

Our list of Foreign Correspondents has sustained only a single loss during the past year; but this, unfortunately, is of one of its most honourable names, *François Jules Pictet.*

Pictet was born in Geneva on the 27th September, 1809. He took his degree of Bachelor of Sciences at Geneva in 1829, and in 1830 paid a visit of six months to Paris, where he made the acquaintance of the great French zoologists of that day—Cuvier, Geoffroy Saint-Hilaire, Duméril, Flourens, Latreille, and Victor Audouin. His intimacy with Audouin, no doubt, had much to do with the direction which his studies took on his return to Geneva, when he turned his attention almost entirely to Entomology, and especially to the study of the Neuroptera, upon which he published several valuable memoirs between 1832 and 1845.

The most important of his writings upon this department of Natural History are:—his 'Recherches pour servir à l'Histoire et à l'Anatomie des Phryganides,' a quarto volume of 240 pages, illustrated with 20 plates, published in 1834, which received in 1832 the prize founded at Geneva by Lady Davy, after the death of her husband in 1829; and the commencement of an 'Histoire naturelle, générale et particulière des Insectes Névroptères,' of which he completed only two monographs (of the Perlidæ and Ephemeridæ),

published respectively in 1842 and 1845. These two monographs alone occupy more than 700 large 8vo pages, and are illustrated with 100 plates, the drawings for which were made by the author himself.

In 1830, under Professor A. P. De Candolle, Pictet commenced giving demonstrations in comparative anatomy in illustration of the Professor's lectures; and this practice he continued until 1835, when the elder De Candolle resigned the Professorship of Natural History, and its duties were divided—Alphonse De Candolle taking the Botanical Chair, while Pictet was nominated to the Professorship of Zoology. Of his success in this new position, M. Soret, from whose memoir of Pictet the materials for this notice are derived, speaks in the most enthusiastic terms. His general course included comparative anatomy, physiology, and zoology, and occupied two years; but in each term he also gave a special course upon some particular subject, such as embryogeny, or teratology, or upon some particular class of animals.

After the revolution of 1846 the Academy of Geneva was reconstructed; and although Pictet retained his Chair, his duties were somewhat modified. The course of human anatomy and physiology was handed over to M. Mayor, and some departments of zoology to the celebrated German naturalist, Carl Vogt, who was at that time compelled to take refuge in Geneva. From 1847 to 1850, through the stormy period following the revolution, when political feeling in Geneva, as elsewhere, ran very high, Pictet occupied the honourable position of Rector of the Academy of Geneva; and it was largely due to his moderation and sound practical sense that the reconstruction of the Academy was judiciously effected, and the rancour of political feeling prevented from having too much influence upon the fate of some of his colleagues. He was Rector of the Academy again from 1866 to 1868, and from 1863 to 1869 a member of the Council of the Federal Polytechnic School at Zürich.

In 1863 he was elected a Foreign Correspondent of the Geological Society.

Besides zealously performing his academical duties, Pictet frequently gave lectures to general audiences upon natural-history subjects; and in these, as in his academical courses, he was eminently successful. But, according to M. Soret, "it was not so much by his lectures, whatever may have been their merit, as by more familiar instruction that Pictet influenced studious young men. He collected the students in the Zoological laboratory of the Museum, giving them

practical demonstrations of his lectures, encouraging them to dissect for themselves, and to make anatomical preparations for the collections," or he made excursions with them into the surrounding country in search of natural objects of interest. Thus, by precept and example, Pictet was the means of raising in Geneva a school of energetic young naturalists, some of whom, especially the late Edouard Claparède, have taken a very high place among the cultivators of science.

The improvement of the Museum of Natural History at Geneva was a constant object with Pictet. As a student he devoted his energies to its enrichment and arrangement; throughout the whole period of his Professorship he laboured incessantly for the same purpose; and, as M. Soret says, in its present state the Museum may be regarded as his work. He added to the collections by large gifts of specimens, and sometimes by assigning to it the proceeds of a course of public lectures, or of the sale of some of his works. The palæontological collection was entirely created by him; and his Cretaceous fossils, which he kept by him for the purposes of study, will eventually be added to the other collections. Under Pictet's management, and partly by his influence, a new building was prepared for the reception of the collections of the Museum; this was barely finished, and the removal of the specimens had only just commenced, at the time of his death.

In carrying on the business of the various scientific Societies of his native city our late Foreign Correspondent also took an energetic part. He was an active member of the Société de Physique et d'Histoire Naturelle de Genève, of the Société pour l'Encouragement des Arts, and of the Geneva section of the Société Helvétique des Sciences Naturelles. To the sections of Neuchatel, Zurich, and the Pays de Vaud he also belonged as an Honorary Member. Of the first-mentioned Society he was President in 1859-60; and in the second he was three times called upon to occupy the position of President of the section of Fine Arts.

In the midst of his scientific and academical labours Pictet found time and energy to fill a tolerably prominent political place among his fellow-citizens. He was for many years a member of the Representative Council of Geneva, in which he remained through the revolution of 1846. In 1862 he was elected President of the Constituent Assembly of Geneva. Before this, he had already been mixed up with the general politics of Switzerland, having been sent in 1855 as a Deputy to the Council of States of the Federal Assembly

at Berne. This position he seems to have retained to the last; for it was while attending the sittings of that body during the revision of the Federal Constitution in the winter of 1871-72 that he met with an accident which must be regarded as the proximate cause of his death. When walking in one of the streets of Berne on the 20th January, 1872, he slipped upon the ice of the pavement, and, in a violent effort to save himself from falling, ruptured a blood-vessel in his thigh. His health had previously suffered owing to severe attacks of sciatica; these now returned with increased violence, and the extravasation of blood from the ruptured vessel was also serious; but after a time Pictet was able to return to Geneva, where his health seemed to improve a little; but when his friends were beginning to rejoice in this amelioration, a putrid fever set in, and carried him off within three days. He died on the 15th March, 1872; and M. Soret says that in Geneva his death was regarded as a public misfortune.

It is as a palæontologist that François Jules Pictet chiefly interests us. It is impossible now to ascertain precisely what turned his mind from its early entomological predilections, and caused him for nearly thirty years to devote almost all his energies to palæontological studies. Perhaps, as M. Soret suggests, the death of several entomologists with whom he was in constant correspondence, and especially that of his friend Audouin, may have deprived his entomological work of much of its charm; perhaps the view of the larger questions opened to his mind by the study of palæontology may have led him captive almost in spite of himself. Among his early papers there is only one of a palæontological nature, namely a note on the bones of fossil bears found in a cavern near Mialet, communicated in 1833 to the Société de Physique et d'Histoire Naturelle de Genève; and we may therefore easily understand that the publication by Pictet in 1844-46 of his 'Traité élémentaire de Paléontologie' excited some surprise in the minds of the geologists of that time. This work consisted of four volumes, with an atlas of plates. The plan of it probably originated in Pictet's mind when, after having made fossils the subject of one of his special courses of lectures, he began to refer to them more and more in his regular course. The want of some handbook of palæontology for the use of his students must soon have forced itself upon him; and he seems to have at once set about the preparation of such a work. As in this book he did not confine himself to indicating the general characters of the systematic groups under which fossil organisms may be arranged,

and their broad distribution in the series of fossiliferous strata, but aimed at giving the characters and stratigraphical range of all the known genera and species of fossil animals and plants, it is clear that even thirty years ago he undertook no light task; and yet the manner in which it was performed was such that his 'Traité élémentaire' at once took its place as a standard work. A second edition of this book was published in 1853-57; and although it is now necessarily somewhat antiquated, we possess no work on general palæontology that can be said to approach it in any way.

The 'Traité de Paléontologie" is especially interesting, not only as showing by the uniformity of treatment of the various groups how wide was the range of its author's knowledge and sympathies, but also as giving us our chief evidence, with the exception of one or two review articles, of the attitude taken by Pictet with respect to the great natural-history question of our day, that of the origin of species. This subject is referred to in the introductory chapter of the work; and in the first edition we find Pictet adopting as the most satisfactory hypothesis the doctrine of *successive creations,* recognizing, however, that there are many facts which seem inconsistent with that doctrine, and admitting that, in some cases, groups of nearly allied species may have originated by the modification of the descendants of a preexisting form. In the second edition of his book Pictet goes a step further and adopts an hypothesis of a gradual transformation of species, as parallel to, and apparently of equal force with, that of successive creations, or, as he now calls it, the "*independence of Faunas.*" "Perhaps," he says, "we must seek the truth in a theory intermediate between those we have discussed, or in a mixture of the two. The theory of independence of faunas should probably be applied to the appearance of distinct types; for these certainly do not originate by means of direct and normal generation from the very different types which preceded them. But, on the other hand, may not the replacement of species by analogous species lead us to believe in transitions and changes of form within certain limits?"

In an article on Mr. Darwin's 'Origin of Species,' published in 1860, Pictet further develops his views on this subject as follows:—"I have always," he says, "regarded the succession of organized creatures as under the influence of two forces. One, which I shall name *normal generation,* is that which acts before our eyes, produces the resemblance between children and parents, and ensures the permanence of the species through numerous successive generations, but

nevertheless implies and permits certain variations which the study of the existing world abundantly shows us. I believe that the long series of geological times may have given this a little more influence, and enabled it, by the accumulation of analogous results, to originate several nearly allied species from a single one. The second, which I shall call *creative force*, acted at the origin of things to produce directly a varied and abundant fauna; and its action, manifested at distant intervals, has successively brought to light those distinct types the existence of which palæontology teaches us." M. Soret seems to be of opinion that Pictet had advanced considerably beyond this standpoint in his later years, but states that he always maintained that natural selection alone does not suffice to explain the facts with which we are acquainted; and in the concluding paragraph of a review of Gaudry's 'Animaux fossiles et Géologie de l'Attique" Pictet himself says "if the transformations should once be proved in their universality, we should prefer to believe that they are dependent upon general laws established ever since the creation, and acting, perhaps, with a periodicity which is unknown to us."

It was not, however, with these general questions, which he evidently did not consider ripe for discussion, that Pictet chiefly occupied himself. In the present condition of natural history he regarded the accumulation of facts as the most important duty of the palæontologist; and he certainly took up this department with immense energy and success. His investigations were directed chiefly to the fossils of the Jurassic and Cretaceous deposits of his native country; and his researches, the results of which his affluent fortune enabled him to publish in the most perfect form, have certainly enriched our science with a multitude of species, well described and figured.

Pictet's earliest palæontographical work of importance was the "Description des Mollusques fossiles qui se trouvent dans les grès verts des environs de Genève," published originally in the 'Mémoires de la Société de Physique et d'Histoire Naturelle' between 1846 and 1854, and afterwards as a separate work under the above title. In it the Cephalopoda are described by Pictet alone; in the working out of the Gasteropoda and Acephala he associated himself with M. W. Roux.

Encouraged by the good reception accorded to this work, Pictet in 1854 commenced the publication of his 'Matériaux pour la Paléontologie Suisse,' consisting of a series of monographs of Swiss fossils, prepared generally by himself, with the cooperation of certain of his

friends and pupils, such as MM. Renevier, Gaudin, de la Harpe, A. Humbert, de Loriol, and Campiche. It is unnecessary to discuss in detail the contents of this great and important series of memoirs, in which Pictet seems to have endeavoured single-handed to do for Switzerland what the Palæontographical Society is doing for this country. The extent of ground covered by the two works just referred to will be best understood from the statement that they contain 3310 quarto pages of text, and 370 plates of illustrations. They relate exclusively to the Jurassic and Cretaceous fossils of Switzerland, and have made known to us a reptilian and molluscan fauna the richness and interest of which were previously not even suspected by geologists.

After these gigantic palæontographical works it is hardly necessary to refer to the other writings of Pictet on kindred subjects, although these were neither few nor unimportant. The most valuable of these papers were collected and published together under the title of 'Mélanges Paléontologiques' between 1863 and 1868; these consist chiefly of notes upon certain groups of fossils from parts of France on the borders of Switzerland.

In the session of 1868, I had the honour of contributing to the Society a paper on the Physical Geography of Argyllshire, in which I ventured to contest the views put forward by the younger school of glacialists in assigning a very subordinate and almost trifling power to subterranean causes in moulding the existing surfaces of the earth. The argument maintained in that paper received in the verbal discussion which ensued a considerable amount of support from Sir Roderick Murchison and from Sir Charles Lyell; and, although strongly opposed by Professor Ramsay, so far as I am aware, that argument has not been met by any answer in detail. But the increased and increasing attention which is given to all questions of quaternary geology, and especially to the causes and to the effects of the Glacial period, has produced since 1868 numerous papers bearing on the subject; and having myself had, since that year, some more extended opportunities of observation, I am induced to revert to the subject, with the view, if possible, of extracting from facts generally admitted some general principles which may guide our reasonings on the subject.

If I may judge from a paper lately contributed by Professor Ramsay to 'Macmillan's Magazine,' upon the valley of the Po, and from the

recent discussion on Mr. J. F. Campbell's very interesting paper on the Glaciation of Ireland, its eems to be admitted by Professor Ramsay that no larger amount of work can be assigned to the glaciers of the Glacial epoch than that of greatly deepening valleys which had existed before. If this be admitted, then the question of the effects of glacial denudation in determining the existing configuration of the surface of the earth becomes a comparatively narrow question. The existence of a glacial epoch, at least over a large part of the northern hemisphere, which in its coming, its duration, and its passing away, has been the latest of the great agencies of change, is perhaps one of the most firmly established doctrines of geological science. And if it be admitted on the one hand, that when that period began it found the existing systems of hill and valley in the main determined, it must also be admitted on the other hand, that it cannot have left them exactly as it found them. The intensity given to denuding agencies by frost, or rather by the alternations between frost and thaw, is well known to be enormous; and it is impossible that a glacial period should have come on, should have endured for a long period of time, and should have gradually given way to a more genial climate, without having left upon the preexisting surface powerful and lasting effects. But the conclusion that the Glacial epoch deepened within certain limits preexisting valleys, degraded to a like extent preexisting hills, filled up estuaries with moraine matter or with sand and gravel, or covered a great extent of country with a Boulder-clay—all this is very different from the conclusion that our existing systems of hill and valley, and even of sea and coast, have been all cut out of the solid by some great ice-sheet of enormous thickness, which was quite independent of local glaciers and which did not derive either the cause or the direction of its motion from the mountains which we now see. Such, as I understand it, is the new glacial theory, which is more or less clearly implied, and apparently almost taken for granted, in numerous recent papers on the subject.

This, however, is one of the many questions on which the conclusions of geology are being corrected and controlled by the results attained in other branches of physical inquiry. It has been well pointed out by Professor Tyndall* that glacial conditions require in the first place an enormous development of heat, and in the second place a corresponding development of high and cold condensing surfaces. This admirable generalization seems to condemn any

* Forms of Water," 1872.

theory which supposes the universal prevalence of extreme cold over the surface of the globe, and likewise any theory which fails to recognize that mountain-chains are the cause and not the consequence of glacial conditions. Existing facts confirm these theoretical conclusions. There are two vast regions of the globe which are now under a glacial epoch. It is constantly assumed by glacialists that the condition of these two regions in the present day affords examples of the conditions which they suppose to have existed in the times referred to; Greenland especially is often quoted as presenting phenomena of the kind supposed; but I am not aware of any evidence which bears out this assumption. Greenland is a high mountain-country, covered more or less completely with snow. But it does not appear to be a country covered with an ice-sheet moving freely over the tops of the mountains, in directions irrespective of their slopes and valleys. I am not aware of any evidence to show that in either the Arctic or the Antarctic regions any such ice-sheet now exists. On the contrary, there seems to be abundant evidence that both the great continents of the globe which are now subject to Arctic conditions present precisely the same features as those presented by every mountain-chain high enough to support a glacier system. In both of them there are lofty mountains to form the gathering-ground of snow, with steep declivities to account for the causes of glacier-motion, and with valleys to control and guide it. Dr. Kane, whose powerful descriptions of the magnificent phenomena presented by the west coast of Greenland are well know, gives emphatic testimony to their substantial identity, differing in scale only, with the glacier-phenomena of the Alps. The shape of the hills is everywhere determining the shape of the ice-streams; and only in a minor degree, and as a secondary consequence, are the ice-streams modifying the shape of the hills. Great valleys, or sweeping hollows, marked by containing walls and buttresses of rock frequently piercing the perpetual snows, bound and define the glaciers towards the coast; and as far as the eye can reach to the lofty summits of the interior, the surfaces of the ice and snow give evidence by every variety of form of the underlying slopes and precipices upon which they are moulded, and over which they are slowly slipping to the sea. Moreover the ocean-sounds into which they fall at last, and which they people with a stately procession of enormous icebergs, are channels lying between coasts whose general trend is at right angles to the direction of the ice-streams which cut them. Those lines of coast must there-

fore have been determined by other causes; and the boundaries of the land, which are, of course, inseparably connected with its elevation, and with the general contours of its surface, are seen to be equally independent of the comparatively superficial work done by the snow which gathers upon its tops, and which is shed from its sides.

The same phenomena, only on a still grander scale, are presented on the northern face of the great Antarctic continent, which was discovered by Sir James Ross. There, indeed, the presence of enormous volcanic cones, in full activity, gives still more striking and emphatic testimony to the close proximity of the great subterranean forces with which elevations and subsidences of the earth's crust are inseparably connected. An immense chain of mountains, with innumerable peaks from 9,000 to 14,000 feet in height, fringes the South Polar horizon for many hundred miles, and is the vast gathering-ground of snows and glaciers of corresponding magnitude and extent. The breadth and conformation of the country which lies between the mountains and the coast, and which determines the peculiar unbroken tabular form of the "Great Southern Barrier" is not absolutely known. But the general character of the whole as the result of the formation and descending movement of true mountain-glaciers, becoming confluent along the foot of the range, cannot be better indicated than in the description given by Sir J. Ross of the scene which the coast presented on one of the last days on which it was clearly seen, February 25, 1841. "We had in the afternoon a good view of the coast. The whole of the land being perfectly free from cloud or haze, the lofty range of mountains appeared projected upon the clear sky beyond them, beautifully defined; and although of a spotless white, without the smallest patch of exposed rock throughout its whole extent to relieve it, yet the irregularities of the surface, the numerous conical protuberances, and inferior eminences, and the deeply marked valleys, occasioned many varieties of light and shade that destroyed the monotonous glare of a white surface, but to which it is so very difficult to give expression either by pencil or description." Sir J. Ross does injustice to his own powers. No artist's pencil could bring more vividly before us the magnificent scene then before him. It enables us to be certain at a glance that no great ice-sheet covers up and conceals the mountains of the Antarctic continent. They are indeed covered by deep snows, but by snows which are moulded by the terrestrial surfaces

below them—which surfaces were determined before those snows had begun to fall, and to the minutest inequalities of which the snowy covering conforms—"clinging" as Tennyson describes the mist in 'Guinevere,' "clinging like a face-cloth to the face." The ice-sheet which exists below is simply the confluent glaciers which are formed in the old terrestrial hollows, and whose motion is due to the steepness of the old terrestrial hills. Where these hills are of great elevation the ice-sheet is deep; where they are low the ice-sheet is comparatively thin (Voyage of Sir J. Ross, vol. i. p. 257). When, in the following year, Sir J. Ross again approached the Great Southern Barrier at a point far distant to the eastward, he specially observes that its perpendicular cliffs had dwindled down to less than half the elevation to which they attained at the foot of Mount Terror (vol. ii. p. 201). And when, at the close of the same year, Sir J. Ross again approached the southern continent in a much lower latitude, to the south of Cape Horn, he tells us of an enormous glacier several miles in breadth, which descended from an elevation of about 1200 feet into the ocean from a small but lofty island; and the vertical cliff which it presented to the sea was at once recognized as the same phenomenon in miniature with the Great Southern Barrier (vol. ii. p. 325).

From observations such as these we may be assured, I think, of the truth of the theoretical conclusion that lofty mountain-chains, with all their characteristic varieties of surface, must in all ages and in all parts of the globe have preceded the development of glacial conditions, and that in those chains the unequal elevations and depressions which are the work of subterranean force have ever been the guiding and controlling cause of glacial action; and it is well worthy of remark that if there is any truth in the idea of the existence of an open north polar sea enjoying milder conditions than prevail further south, there is every reason to suspect and to believe that these milder conditions are due to no other cause than the absence of land with high mountain-chains.

No doubt, when glacial conditions have been developed, the movement of masses of ice which is due to their descent down steep declivities is of a powerfully abrading character upon the surfaces which are exposed to their passage. But, on the other hand, the protection which a universal covering of snow upon the highest surfaces affords against the other agencies of denudation must be very considerable, and fully explains the contrast remarked upon by

Professor Tyndall* between the broken and splintered aspect of the rocks and peaks which have risen above the ancient glaciers in the Alps, and the smooth and polished surfaces which were below the ice and have been thus protected against every other destructive agency than its own. So far as I am aware there is no evidence that snow can scratch rocks or that it can do so even in the half consolidated state which is known in the Alps under the name of Nevé; yet this must be the condition of a large part of the snowy country on the higher levels of Greenland and of Victoria Land.

seems probable, indeed, from every description of Arctic and Antarctic phenomena, that the destruction which is now being effected in those regions of the globe is much greater upon the rock-surfaces which rise above the glaciers, or which are not directly exposed to them at all, than upon the surfaces over which they pass. The rate of disintegration upon naked rocks which are exposed to alternate frost and thaw, and especially upon the cliffs which front the coast, seems to be enormous; and in cases where the subjacent material is so soft or so broken up as to be capable of being lifted by ground-ice and incorporated in the body of a passing glacier, the removal of material may also be very great. But, on the whole, I am inclined to think, with my distinguished predecessor in this Chair, that the power of denudation, whether from ice or from other agencies, "is a power of planing down exposed surfaces rather than that of excavating very deep troughs." (Address, 1872, pp. 60, 61.) The preexistence of valleys and hollows for the accumulation of the ice necessary to the formation of a glacier seems as necessary as the preexistence of the summits which condense and collect the snows. But when such hollows and ravines have once been formed, and after they have come to be thus occupied, there can be no doubt that they must be cut deeper, with a rapidity proportioned to their own initial depth, which is the measure of the occupying mass, and to the steepness of their descent, which is the measure of the rapidity of the movement. But in every one of these matters the governing conditions are those which precede glacial denudation, guide it, and limit its effects.

And here I must observe that if our existing systems of hill and valley are indeed due to glacial action, that action must have been coincident with the existing lines of drainage, and no action can be quoted as proof of this effect which has been transverse to those

* Forms of Water.

lines. Valleys cannot have been cut out by an agency which has not worked along them but has cut across them. The contours of mountains cannot have been determined by any force which has worked irrespective of the valleys in which and out of which those mountains stand. And yet we hear constantly of ice-marks which are totally independent of the existing drainage—marks which indicate the passage of masses not coming from any existing summit, and not travelling down any existing hollow. These masses are supposed to have moved up hill and down dale, in some definite direction at every variety of angle with the existing lines of mountain and of valley. I am not now disputing the accuracy of these observations. I have myself seen marks of glaciation which certainly did not seem to have been due to any local glacier, and I have no reason to doubt the evidence on which we hear of deep and powerful striations which run across the tops or the flanks of hills, and across the lines of valleys. But then I venture to submit that these valleys cannot have been cut out by masses of ice which, so far as they cut at all, must have cut in a different direction.

In the interesting and elaborate paper on the Glaciation of North Lancashire, which has lately been contributed to this Society by Mr. Tiddeman *, his conclusion is that, "whereas the drainage of that district is to the S.W., the general movement of the ice over it was to the S. or S.S.E. across deep valleys and over hills of considerable elevation." The drainage-system which has been thus traversed by the ice of the Glacial epoch cannot be ascribed to the excavating power of that ice; and the description given in the same paper by Mr. Tiddeman leaves no doubt as to the deeper-seated causes by which that system has been determined. Its southern boundary is put down upon his map of the district as "The Rossendale Anticlinal." Its eastern and north-eastern boundary is the Pennine chain, with peaks between 2000 and 2400 feet in elevation. This high country is separated from the western and south-western portion of the district by a broad valley which runs, as Mr. Tiddeman specially mentions, along "The Craven Faults." The Ribble runs along an anticlinal in Carboniferous strata. These are significant indications. They imply that the coincidence which is wanting between the features of the country and the lines of glacial denudation, is a well marked connexion between these features and the lines of subterranean disturbance. In like manner it appears from a

* Quart. Journ. Geol. Soc., Nov. 1872.

paper by Mr. Croll, in the 'Trans. of the Edin. Geol. Society' for 1869, upon ancient river-channels now filled up by great depths of glacial drift, that the great depression which connects the valleys of the Clyde and Forth, along the line of the Clyde-and-Forth Canal, had in preglacial times the same direction which it now has, and was occupied by streams which indicate the same system of drainage as that which now exists.

I have already explained that I do not seek to dispute the fact that many marks of glaciation are to be found which it is very difficult or, it may be, impossible to connect with a system of local glaciers. This difficulty, however, seems very often to be made greater than perhaps it is, by foregone conclusions with respect to a universal ice-sheet. My main object, however, is to point out that the greater this difficulty is, the greater also is the impossibility of ascribing to glacial denudation the existing configuration of our hills and valleys. Yet I cannot but observe, in passing, that there appears to be considerable inconsistency in the views of those who dwell most on the non-local character of glacial striations. It is impossible, for example, to look at the map attached to Mr. Tiddeman's paper on North Lancashire without seeing that the striations of that district, although they lie across some ridges and some valleys, have more or less consistent reference to the higher peaks of the Pennine chain. The difficulty of course is, first, that this chain may not be high enough and near enough to give motion to a glacier of such size as would be required, and, secondly, that it must have moved across intervening hollows. In one case Mr. Tiddeman mentions scratches which would imply a glacier 8 miles broad and 750 feet deep; and this is a conclusion which he considers too startling to admit; but no amount of breadth and depth in ice-masses is considered the smallest difficulty when the machinery of the ice-sheet is called upon for an explanation of similar phenomena. Again, the influence of the mountain at Ingleborough, 900 feet above the level of some scratches to be accounted for, and only 7½ miles off, is set aside as incapable of having made itself felt at that distance; but no difficulty seems to be experienced in supposing that the mountains of the Scandinavian peninsula or Greenland, distant from 500 to 1200 miles, made themselves felt all over Scotland and the north of England in causing the motion of one great ice-sheet coming from the north.

On the subject of this theory as accounting for glaciation transverse to the existing valley-system of the country, I cannot

help expressing great doubts as to the mechanical assumptions which are involved. Glacier masses are supposed to have filled up the entire bed of the North Sea and the German Ocean, to have moved freely up the slopes of their bed, to have ascended the land wherever it was encountered, and to have walked up the escarpments of mountains more than 2000 feet high. We know that the descent of a glacier even down the steep declivities of Mont Blanc is retarded by such an enormous amount of friction that the coherence of its substance is overcome; the base of it is, as it were, torn from its superincumbent mass, and the progress of the base is reduced to one half of the rate at which the surface moves. We know that this is the result in a case where the force of gravity is at its maximum, and none of its momentum has been lost. We know, also, that in no part of the existing world is the phenomenon presented of ice-streams moving for great distances even over level ground, still less ascending steep gradients, and this too at a great distance from the declivities which give impetus to forward motion. Mr. Croll, indeed, admitting this fact, accounts for it by the allegation that both in the Arctic and Antarctic regions the seas are so deep that the power of flotation is called into action close to the existing coasts, and the great ice-sheets thus become broken up into icebergs *. But this explanation does not seem to be consistent with many of the soundings recorded by Sir J. Ross close to the face of the most stupendous ice-sheet now existing on the globe. On the contrary, it appears that within three or four miles of that face the sea was sometimes not deeper than 570 feet, whereas Mr. Croll estimates the Great Southern Ice-sheet at nearly a mile in thickness, or upwards of 5000 feet †. If this be so, the Antarctic Ice-sheet has abundant opportunity of moving northwards along the bed of the Antarctic Ocean, which appears from Sir James Ross's accounts to be under 2000 feet deep for vast distances along the face of the icy cliffs, and is of course too shallow to float an ice-sheet even approaching the depth assigned to it by Mr. Croll. And although I cannot keep thinking that this estimate is much exaggerated, inasmuch as the face of the ice-cliff is nowhere described by Sir James Ross as exceeding about 300 feet in height, and at the point where it was actually measured was only 150 feet high, yet this unquestionably represents a mass of ice too deep to be floated in the shallower soundings which were abundant along the

* Croll, "Boulder-clay of Caithness," Geol. Mag., June 1870.

† Ibid.

coast. At some places, between three and four miles from the ice-cliffs, water was found as shallow as 246 feet; and even at the great distance of 120 miles the bottom was reached at 1140 feet. At some points the deepest soundings were found near the ice-cliff, as where 2460 feet was found within twelve or fourteen miles (January 29, 1841). But as even this depth is inadequate to float such an ice-sheet as Mr. Croll supposes to exist, it ought, according to his theory, to be moving along the bottom to the northwards, and ascending the slopes of the trough, which appear to rise into shallower water to the north.

I am not myself able to attack the problems involved in this theory with those weapons of mathematical calculation which are necessary for the purpose; but I cannot help thinking that there is a fundamental fallacy in comparing the movement of ice-masses down the slopes of a mountain with any movement of such masses which is possible on level ground or against opposing slopes. In the one case gravity is an assisting, in the other case it is a resisting force. In the one case the heavier the mass of ice the easier and the faster will be its motion; in the other case every additional ton must add to the difficulty of effecting movement. In the one case thrust and gravity act together; in the other case thrust must act alone, with gravity and friction to counteract it. And then it must be remembered that, in the case of mountain-glaciers, the thrust itself is due to superincumbent weight—being the result of the accumulation of snow on the mountain-tops; whereas it does not seem clear how superincumbent weight upon level ground, and at a distance from mountains, could have any effect in producing horizontal motion: and if such effect were produced, it would probably be expended in a slipping of the ice over itself, rather than in any progress of its base over rough and ascending surfaces of rock. I have observed that in papers involving the extreme theory of glaciation, there is a general avoidance of any explanation as to the cause of motion in the assumed ice-sheet; and I have reason to believe that the experiments and reasoning of Canon Moseley on the causes of motion in ordinary glaciers have left the impression on some minds that he has disproved the theory which ascribes that motion to gravity. As this would be a merely negative conclusion, it would leave room for unlimited speculation as to the causes to which ice may owe a power of movement; and the result is that some geologists have a vague idea that as true glaciers do not move by weight, but nevertheless do move in some unexplained

manner, so the supposed ice-sheet may have moved in the same mysterious way. But I apprehend that this is an entire misconception of Canon Mosely's argument: what he has proved is, not that glaciers do not move by their weight, but that they do not move by weight alone—that is to say, unaided by other forces. But I apprehend that the action of gravity is an essential element in his theory of glacier motion, and, moreover, that it is the one essential force which determines the direction of the movement. That movement is facilitated and rendered possible against the tremendous friction of the bottom ice upon its bed by the alternate conditions set up by variations of temperature acting on the molecular structure of the ice. But it would be altogether illogical to suppose that because these molecular changes are able to overcome friction when they are powerfully assisted by the gravity of the mass lying on a steep slope, therefore they are equally able to overcome friction with no such assistance from gravity, but, on the contrary, with gravity exerting all its force in favour of rest and against motion of any kind.

Falling Water! travels to a level So does Ice

On these grounds I see many difficulties in the way of accepting the theory which ascribes the glaciations which cannot be traced to local glaciers to some gigantic ice-sheet, which is supposed to have moved as land-ice never moves now and, as there is strong reason to believe, never could have moved at all—that is to say, at enormous distances from any mountain-slopes, and up other mountain-slopes, which it is assumed to have ascended easily.

The only passage I have met with in any paper assuming the extreme glacial theory, which attempts to define the conditions under which an ice-sheet can ascend hills, is a passage in a paper by Professor Agassiz on glacial phenomena in Maine (reprinted from the 'Atlantic Monthly,' Feb. and March, 1867). Professor Agassiz says, "In order to advance across a hilly country and over mountainous ridges, rising to a height of twelve and fifteen hundred feet in the southern part of the state, and to a much higher level in its northern part, the ice must have been *several times thicker* than the height of the inequalities over which it passed; otherwise it would have become encased between these elevations, which would have acted as walls to enclose it. We are therefore justified in supposing that the ice-fields, when they poured from the north over New England to the sea, had a thickness of at least five or six thousand feet." This is vague enough; but at least it shows some appreciation of the difficulty in dynamics which is involved.

The Tide flows up hill in the Channel

"Several times thicker than the hills which are surmounted" may mean three, or four, or five, or six, or seven times; and no data are given to enable us to guess what height of ice over inequalities will enable it to surmount them. Nor is the question touched whether, even in the supposed case of ice-sheets six, seven, or ten thousand feet thick passing over mountains of 3000 feet height, the ice so moving would be confined to the upper stratum, leaving the lower fixed and imbedded in the hollows and the valleys. And if this were the case, then no striation would result in those valleys, and any striation which exists must have some other explanation.

The phenomena of striation are undoubtedly perplexing; but I question very much whether there are any of them which are really incapable of explanation, if we suppose a subsidence and reelevation of the land under glacial conditions to the depth of about 2000 or 3000 feet. Every portion of the land would, during this series of operations, and up to the supposed height, be subjected to the action of ice in the three great forms in which its action is known to us in the present day in Arctic and Antarctic regions:—namely, first, true glaciers formed on preexisting mountains filling preexisting valleys, and spreading out more or less in ice-sheets whose motion is due to glacier-thrust: secondly, to icebergs alternately floating and grounding, pounding, smashing and confusing the broken materials of the sea-bottom, and covering it with transported blocks: thirdly, enormous floating ice-sheets, or floe ice, which by movement of tides and currents, are frequently heaped and piled against opposing surfaces with gigantic force, and to considerable elevations. That floating ice of this kind can and does produce striation upon such rock-surfaces will not, I presume, be questioned. The evidence of it in my own neighbourhood is abundant. On almost all the projecting points along the upper shores of Loch Fyne there are deep and powerful striæ, running parallel with the bed of the Loch, and extending from the present level of the water to about 15 or 20 feet above it. The ice which made them has not been moulded upon the rock-surfaces, fitting into their inequalities, as an ice-sheet resting upon them would have been fitted into them. It has been pressed only against the faces exposed to the direction from which the ice came, leaving the sheltered side unstriated—the natural results of the comparatively rapid and impulsive movement of floating ice-sheets. As regards the power of icebergs to produce striations upon rocks at the bottom of water in which these vast masses are just afloat,

and no more, and in which they are impelled by currents, there can, I imagine, be quite as little doubt. We have only to remember that Sir J. Ross saw an iceberg in the Antarctic Ocean suddenly turn upside down and expose its lower surface, so completely covered and ingrained with stones, sand, and gravel, that for some time it was thought to be an island not before observed. If such a berg as this were carried grating over a rock-surface at the bottom of the sea, its abrading effects would be enormous. Nor would its power be less of ploughing up and confusing loose materials which had previously been arranged by water. It is possible that to this cause may be ascribed many (I do not say all) of the accumulations in glaciated countries, which exhibit a complete confusion of the arrangement due to specific gravity. But whether the confused heaps of scratched stones, clay, mud, and gravel which are so common have or have not been due to the ploughing, pounding, and pushing movements of icebergs, half afloat and half aground, there is one phenomenon prevalent all over Scotland, which cannot, I think, be ascribed to any other cause than that of floating ice. I hold it to be as certain as any conclusion upon the subject, that the perched blocks which crown so many hills in Scotland have been deposited there by icebergs grounding upon tops which were then shoals and reefs in a glacial sea. And every portion of these hills, as they emerged from that sea, would be successively exposed to the grinding action of floe ice. I have lately examined some fine perched blocks, and some blocks also in the hollows of glens, which, by their position and structure, give the clearest evidence of having been let down through water, because they sit upright with their heavier ends downwards, and their broad surfaces vertical. The great elevation at which some of those perched blocks are found, indicates the extent to which submergence has gone in some part of the glacial times; and if glacial conditions continued to prevail during the whole process of emergence, floe-ice, impelled by currents which may have been persistent in direction during long periods of time, would have access to every part in succession of the rising land.

How far the preexisting configuration of hill and valley may have been modified during these vast movements of subterranean force it does not seem possible to estimate; but it is obvious that much glacial striation would take place across submarine reefs, as well as much which, though generally parallel to ridges and hollows determining the course of currents, would yet be such as could not have been made by land-glaciers.

Whether this be an adequate explanation or not of the glacial marks which are transverse to our existing valleys, and which have cut across the flanks of our existing hills, I return to the point that these marks cannot indicate the process by which those hills and valleys have been formed. They indicate an agency by which the hills may have been somewhat degraded, and the valleys somewhat filled up. But they do not indicate an agency by which the valleys can have been hollowed out. This can only have been done by true glaciers, of which preexisting summits were the gathering-ground, and preexisting hollows were the bed. And for those preexisting and still surviving systems of hill and valley, we must go back to older and deeper causes than the comparatively superficial operations of tho Glacial epoch.

It may be true, and indeed it may be considered certain, that the forms of hill and valley which thus preceded the coming on of glacial conditions had been themselves determined in a large degree by previous denudations arising from other agencies; and those who see in every thing the effect of the Glacial epoch are under a very natural temptation to appropriate to one period, and to one cause, effects which have really been due to many different epochs and to many different causes. No geologist doubts the enormous amount of denudation which has affected the surface of the earth during the course of geological time. We all know that denudation and deposition are inseparable correlatives, and that the amount of destruction done upon the older rocks is measured only by the enormous masses of sedimentary deposit which have been derived from their materials. But the recognition of this fact goes but a little way in determining the causes which have operated in throwing some of the newest as well as some of the oldest rocks into the forms which now mark the more hilly or mountainous regions of the globe.

In order to judge of this we must look into the details of structure, and the relation which internal structure bears to external form.

There is one fact of structure which is of immense significance; and that is, that for the most part the older sedimentary rocks have had the original position of the beds greatly changed, and that this change has been such as to have the general effect of greatly reducing the superficial space which they must once have occupied, and which they would still occupy if they were again spread out horizontally. This effect is produced by the beds being

thrown out of the horizontal, and more or less into the vertical direction, or by their being crumpled into folds. A forcible description of this fact as affecting the Palæozoic rocks which stretch underground from the Mendips in England to the Ardennes in Belgium, is given in my predecessor's last annual address. These old rocks are described as "crumpled and contorted for a breadth of many miles, and along a length of above 800 miles. The Silurian and Devonian rocks are thrown up into a number of narrow anticlinals; and the flanking coal-measures are tilted, turned back on themselves, squeezed and contorted." Mr. Prestwich further describes these "contortions as so great that the crown of the anticlinal, which rises in Belgium, is at the height of four or five miles above the level of the reversed synclinal arch to the bottom of which the coal-measures descend." And Mr. Godwin-Austen is quoted "as having estimated that the Belgian coal-measures have been squeezed into a space one fourth only of that which they originally occupied." There can be no doubt of the cause of crumplings and foldings of strata such as these. They can only be due to lateral pressure, and this lateral pressure could only arise from the strata losing their subterranean support, and being therefore compelled to adjust themselves to the occupation of a narrower space. This is a process that must have been frequently repeated, if there be any truth in the physical doctrine of the earth having been subject to a secular cooling from a higher degree of internal heat; and as often as it has been repeated it must have produced upheavals and depressions on the surface. However slow such flexures into hill and valley may have been, the work has occupied in each case a definite time, which, within certain limits, can be defined. In the case of these Palæozoic hills and valleys now so deeply buried by sedimentary deposits of later origin, we know that they were formed after the deposition of the Coal-measures, and before the deposition of the Permian strata. It may well have been that when this great axis of disturbance arose in the Silurian and Devonian deposits, the Coal-measures may have formed the surface of the ground; and they may have existed in much greater depth than they have anywhere remained. Nevertheless they had to follow the movement of the rocks on which they rested; and they were accordingly tilted, squeezed, contorted, and in some cases actually turned back upon themselves.

These buried mountains are, I conceive, the counterpart of other ranges which have not been buried or have been again uncovered.

The immense area of country, for example, over which in the highlands of Scotland one general direction of dip and of strike prevails is conclusive evidence of great flexures and great faults having been caused by subterranean movements. I have described, in a former paper communicated to this Society, how in my own neighbourhood the mica-slates have fallen inwards along lines of subsidence, having a general north-eastern and south-western direction, and how the porphyritic granite can be shown to have risen along the planes of least resistance, which were the planes of deposit in the falling beds, so that the granites appear to be interstratified in masses with the slates, but indicating their intrusive character by the fragments which they have caught up and enveloped. All the mountain-ridges of the country lie along the strike of highly inclined beds; and the most jagged and peaked outlines are at points where the strata have been most bent and contorted.

It is, as I conceive, after such movements as these have been effected, and after they have determined the great anticlinals and synclinals of mountain-systems, that the forces of denudation begin to work, and to work upon them to the best advantage. Whether under the effects of subsequent gradual submergence, or the equally powerful effects of gradual emergence, strata so contorted, bent, broken, and turned back upon themselves must offer a thousand points of advantageous attack to the innumerable agencies of waste and degradation. But so long as such hills and mountains last at all, and wherever they are exposed to view, they bear upon them the unmistakable impress of their origin, and of the mighty subterranean forces to which their structure is due. But structure generally determines the whole aspect of the country where mountains exist. Even their distant outlines betray at once, to the experienced eye, the direction in which the strata have been tilted, and very often also the direction in which elevations have taken place along the line of strike. Denudation has indeed done its work; but it has done it along the lines determined by upheaval, by fracture, and by unequal subsidence. And it must be remembered that there are many positions into which strata may be thrown on which a comparatively small amount of denudation might result in a surface having no apparent connexion with structure. All sedimentary beds must have had an edge somewhere; and if they are tilted into a vertical position, and the edges come to be exposed, the removal of a small amount of material may result in a horizontal surface, or in surfaces cutting across the lines of struc-

ture at every variety of angle. And where the removal of material has been greatest, as in some cases it has been enormous, the commencement and the direction of the work done by the denuding agents have been determined generally by the lines of flexure, which are the lines of greatest tension, and therefore the most probable lines of fracture. Even in the case of mountains which have most the appearance of having been, as it were, cut out of the solid simply by the removal of all the surrounding material, it will generally be found that these great "monuments of denudation," as they are often called, stand in the bottom of synclinal troughs, or at the top of anticlinal arches. Frequently, too, the core of these mountains is some great column or mass of intrusive rock which has risen from below. If we may trust the beautiful map of the north of Scotland by Murchison and Geikie, this is the case of Ben Nevis, and probably also of Ben Lawers, which are both of them mountains standing in deep troughs, or on the edge of extensive faults. The Linneh Loch is an extension of the trough of the Caledonian Canal; and that trough is due, as we are informed by the same authorities, to the double effect of an anticlinal broken through by a coincident line of fault. There is evidence, no doubt, of the removal of great masses of material; but there is evidence also of subterranean movements which have not only certainly determined the direction of denuding agencies, but have probably begun and facilitated the work by extensive fractures.

The mountains of Cambrian sandstone, which are such wonderful and picturesque objects on the west coast of the counties of Inverness, Ross, Cromarty, and Sutherland, are commonly and justly referred to as among the most conspicuous examples of denudation in the British Islands. The dissimilarity of their material to that of all the rocks with which they are associated, and the apparent horizontality of their strata, in contrast with the almost vertical beds on which they stand, and the highly inclined unconformable beds by which they are again geologically succeeded, suggest to the commonest observation the question, how these remnants of vast sedimentary deposits can have assumed the form in which we now see them, and how the rest of that vast mass has come to be removed. To approach even to the solution of the problem, how much of this has been due to subterranean, and how much to subaerial force, we should require to know, in outline at least what have been the movements of upheaval and of subsidence since the immensely remote geological time when they were deposited—

how much was done during their first emergence from the seas in which they were deposited, whilst the materials were yet soft and unconsolidated—how often and how long they have been elevated and exposed to subaerial action after they had become rock—and, above all, what have been the conditions of upheaval as regards the area and the axes of disturbance. Elevations or depressions which take place very slowly and over a very large extent of country may have little or no effect in breaking, or even in bending strata—whereas movements comparatively sudden and confined to limited areas may produce the most violent fractures, flexures, and fallings-in of the beds exposed to them. As regards these old Cambrian sandstones, I venture to think, from my own observations lately made, that there is evidence of subterranean movements, within a very limited area, which threw the Cambrian beds into some great dome or arch, exposing them to tension too great for them to bear without fracture and dislocation. I think I could plainly see, on the occasion of a late visit to that coast, that the appearance of horizontality in the beds is an ocular deception, from so many of the precipitous faces being sections at right angles to the dip, and from many of the mountain masses having occupied positions near the centre of the great dome-shaped form of which they are the remains. Moreover I think there is evidence that the depression now occupied by the waters of the ocean in the Minch was the line of subsidence into which one side of the dome sloped and fell. Along the margins of the coast there are bits and fragments left which belong, I believe, to the same chocolate sandstones and conglomerates which constitute the peaked or jagged mountains further inland; and these remaining fragments are seen generally dipping steeply into the sea, and presenting thin abraded edges at a high angle towards the hills beyond. There is, indeed, one very large fragment, constituting the well-known Island of Handa, on the coast of Sutherland, which seems to have been lowered, as it were, by some unequal subsidence into a reverse position, with its escarpment facing the Minch, and its slope towards the mainland. But the Laurentian gneiss of this part of Sutherland, on which the Cambrian sandstones rest, gives such abundant evidence of deep-seated subterranean disturbances that every rock resting upon it must be expected to present a differeuce of position. The extent of these disturbances is well exhibited in the general character of the country as viewed from any of the hills, some of which are capped by small remaining fragments of the bottom Cambrian

conglomerates, whilst a bit in the upper beds of the same formation is seen far below our feet in the wave-worn precipices of Handa. One of these fragments is situated at the summit of Ben Stack, a mountain of Laurentian gneiss, upwards of 2000 feet high. The general level of the gneissose hills is very much lower, but deeply broken, in a manner which can only be explained by unequal elevations and depressions along axes of movements frequently recurring and frequently changing in direction. The larger valleys are occupied by comparatively insignificant streams; and many deep hollows are not occupied by any streams at all. The overlying beds of white quartzite present phenomena which seem to me to give ample proof of violent fracture. There is one precipitous face of this rock, superficially coloured red by oxide of iron, which can be traced though some fifteen or twenty miles of country traversing high transverse ridges, and cut through by deep arms of the sea, which cross it at right angles. At one spot on the top of a ridge, between the valley which is occupied by the chain of fresh-water lochs between Loch Shin and Loch Laxford and the still more striking valley which is now occupied by the ocean in Glen Dhu, the ground is encumbered with the broken blocks and masses which indicate the former extension of the same bed to a considerable distance from its present face. No denuding agency that I can conceive will account for the formation of this great line of fracture, although the vast hollow which it now almost encircles, and which it probably once covered, is a sufficient indication that after the fracture had taken place, the broken materials have been somehow removed. It is possible, however, that this line of precipice may be very near the original edges of deposit; and in this case the amount of removed material need not have been so very great.

I may take this opportunity of recommending any member of this Society who is interested in seeing the most magnificent natural section of a vast series of formations which perhaps this island or any part of the world presents, to visit Glen Dhu, in Sutherland. From the comparative nakedness of the hills, their precipitous character, and the accurately transverse line which the bed of the fiord occupies as regards the dip of the strata, and from the depth to which it cuts them, there is presented to the geologist a section of every bed from the Laurentian gneiss to the upper Silurian slates. The Cambrian sandstone is represented by the lofty mountain of Quinaig rising over the mouth of the fiord; and

all the beds of the superior Silurian strata are seen in their order as we proceed towards its head, especially towards the termination of its southern branch. I may also observe, in passing, that a large series of beds of mica-slate and quartz-slate appear to lie between the great white quartzite beds and the Cambrian sandstones, whereas in the sections given by Murchison and Geikie, the white quartzite is the rock which overlies the Red Sandstones.

With respect to the effects of glacial action upon the intensely hard and highly crystalline rocks of the Laurentian gneiss, we may at least assume that, as compared with the other rocks over which it was exerted, that effect must have been about a minimum. Nevertheless there is no mountain-country that I have ever seen which is to be compared with the part of Sutherland occupied by that rock in point of what may be called the tumultuous character of its surface. Nor are the tops of the hills, high or low, in general rounded. On the contrary, they are generally full of sharp abrupt outlines. He would be a bold theorist who would ascribe the form of Ben Stack to the removal by glacial denudation of all the lower country of the same rock out of which it rises.

As regards the theory of Professor Ramsay in regard to the origin of lake-basins, that they are due to excavation by glaciers, I have only to observe that the argument in relation to them stands in the same position as the argument with respect to all other valleys. The mountains and the valleys have preceded glacial conditions, which again have more or less deepened the hollows along which glaciers moved. If valleys so deepened have afterwards been affected by subterranean disturbance of their former levels, they may have become holes and depressions. But I confess I am at a loss to understand how glaciers could stop their onward motion and expend their energies in digging deep holes far below the level of the general slope upon which they lie, and down which they move. It seems to me as reasonable to suppose that the great depression in which the valley of the Jordan lies, descending far below the level of the ocean, has been cut out by a stream which had its outlet in the sea. Take the case of Loch Lomond. Here we have a great valley, the bottom of which is at some places 600 feet below the level of the sea. If the floor of that valley was at a much higher level when the glacial period began, ice moving down it from the higher ranges of Glenfalloch, would follow the line of least resistance to the open country and the Firth of Clyde. There is no conceivable circumstance, except one, which could

cause the glacier to expend its force in the direction of excavation instead of the direction of advance. The one circumstance which might possibly explain the descent of a glacier into such a depression is, that the depression existed before, in which case, of course, it would be occupied by ice, just as it is now occupied by water. But what would be the behaviour of that portion of the glacier which had been so imbedded in a rocky hole? Would it move as freely as a glacier lying on a level or on a descending slope? Is it quite certain that it would move at all? Would not the friction and resistance be so enormously increased as to bring this lower portion of the glacier to rest, and allow the upper part of it to slip over the lower part, as we know it actually does even where the lower ice is not thus imbedded? I propound this as a question of dynamics. It is quite certain, I apprehend, that a rocky depression of some certain depth and thickness would hold absolutely fast any mass of ice which had been forced into it, and that any superincumbent mass of ice in motion over it would have to part company altogether with this imbedded portion—even as the upper part of the *mer de glace* does now part company from its own base by a double rate of motion. And has any calculation been made of the gradient, so to speak, at which ice would thus be brought to a standstill, and would thus absolutely cease to work even as an abrading agent? Until such calculation has been made, the theory which ascribes the digging out of the deep rock-basins to the action of moving ice is a theory which omits altogether to take into account an essential element in the problem which it aspires to solve. Some very curious and interesting investigations into the character and composition of the water in the deeper parts of the lake-basins of Scotland have lately been made by Sir Robert Christison. And one of the results of this investigation has been to show that even the water which gets settled in the deep holes of which I have been speaking, movable as water is in comparison with ice, becomes motionless in those rocky depressions. Speaking of experiments made in Loch Lomond upon the chemical composition and upon the temperature of the bottom water, Sir Robert says, "the results of both concur in indicating that in the very deep parts there is a vast body of still water, which undergoes little or, perhaps, no change or movement." As regards the problem of the formation of such rock-basins I have never quite understood why it has been considered so difficult. It is perfectly true that it is a very rare thing, perhaps (as has been said by Professor Ramsay) the

rarest thing in nature, to find a rock-basin formed by the bending of a bed into a trough-like form. But if mountains and valleys have in the main been formed by the falling-in of rocks, owing to subsidences and lateral pressures of every degree, taking effect upon materials of every variety of texture, it is almost impossible to conceive how such movements could take place without resulting in deep local depressions, just as they have resulted in high local elevations. The one result is the correlative of the other. The deepest part of Loch Lomond is along the base of Ben Lomond; and there seems every reason to suppose that the same causes which have formed the slope above have also formed the slope below the level of the water.

Nor, on this theory of the origin of such depressions, ought we to expect any uniform relation between the shape of hollows so formed and the lie of the beds which are their containing walls. Faults and fractures not only may but must take place at all varieties of angle with the dip or strike of the strata which are broken through; and if in any case those strata have fallen inwards along some line of subsidence, they would so fall often unequally and in such complete confusion as to render it most difficult to account for the appearances presented, especially when these appearances have been complicated by more or less extensive subsequent denudation, and also by frequently repeated subterranean disturbances. I believe this to have been the case with the basin of Loch Awe. The mica-slate beds on either side are there seen plunging steeply towards the basin; and in the little islands in the bed of the lake those strata are seen to be nearly vertical. When the evidence of subterranean subsidence is so clear and conclusive as in this case, I see no difficulty which requires solution in accounting for the formation of lake-basins. On the other hand, there may be cases where such basins do not present the same phenomena, where they appear to have been cut or scooped out of horizontal beds (just as some dry valleys have been excavated) by marine denudation. But the buried river-courses which have been discovered filled in with glacial drift, some of them lying deep below the present level of the sea, suggest that the work of ice has been rather to fill up such hollows than to dig them out, and suggest further that before the commencement of the glacial epoch the mountains with their valleys stood much higher than they do now, and that deep basins, such as Loch Lomond, may be nothing but valleys which were once upland and have since

been lowered unequally. I do not, however, mean to dispute, nor am I disposed to doubt, that there are conditions under which ice charged with stones and gravel could exert some digging-power. If the hardest rocks yield to the slow mechanical attrition of the lithodomous mollusks, they would assuredly yield also to the not dissimilar agency of ice with imbedded sand, if it could be applied to the same work in the same or in any similar way. But I think the conditions must have been rare indeed in which ice could have been applied in any manner at all comparable. Enormous icebergs, just afloat, and kept spinning round by tides and currents for long periods of time upon the same rock-surfaces, would perhaps be adequate to the task of grinding holes, but hardly holes of the shape of our deep lake-basins. These are generally much more like the form of linear cracks or chasms; and if ice ever filled them, which it must have done if the valley in which they occur was ever occupied by a glacier, I believe that the ice so situated must have been completely jammed, and rendered incapable of forward movement. Nor can I see any evidence of lake-basins having been excavated by ice in the fact that the striations observable along the shores are generally parallel with the longer axis of the occupying lakes. This would of necessity be the case on any theory—because, whether those striations were made by glaciers or only floating ice, and whether the valleys were first cut out by ice, or only occupied by it after they had been already formed, the direction of movement would be equally parallel to the valleys as they now exist. A bar or a block of iron which has been polished with sandpaper in the direction most natural and convenient for that operation would exhibit, under a glass, powerful striations in the direction of its longer axis; but it would hardly be safe to infer from this that the block or the bar had been cut out of the solid by the sandpaper. That glaciers have deepened depressions now occupied by lakes, as well as depressions which are now only the beds of streams, there is indeed no reason to doubt. That they may also, at points where their onward progress has been checked by obstructions, have exerted a more than usually grinding action, is probable. But it is remarkable that the rock basins, attributed in this theory to glaciers, do not for the most part lie in the parts of valleys which are narrow or obstructed, but, on the contrary, in general where valleys open out to the broadest extent, and either debouch upon the plains or enter a country with minor hills. This is the case with the Lakes of Geneva, Maggiore, Garda, Como,

and Lugano, and, in our own country, with Loch Lomond and Loch Awe. I may add that, with regard to the thousand little lakes, or tarns, which are common in the Highlands, and literally honeycomb the whole face of the country in some parts of Sutherland, very many of them occupy positions which it would be very difficult to explain by any reasonable theory of the spots which would be selected by glaciers for the exercise of their digging-powers. Many of them in Argyllshire are on the very tops of hills, and almost all of them in hollows close to the summits —situations where, on any possible theory, the weight and working power of land-ice would be at its minimum. On the whole, therefore, I am inclined to think that however much enormous glaciers may have deepened the hollows now occupied by lakes, they cannot have dug them out in any case in which they exhibit any considerable depth below the general level of the slope in which the glacier lay. This, however, is assuming that no considerable change of levels from subterranean movements has taken place since the glacial epoch. But if the land was much more elevated during that period, has been since submerged, and has again been raised more or less, then it is quite possible that what was once only the natural continuation of a slope, may have been converted by unequal movements, and local axes of disturbance, into rock basins, under the conditions which we now see. In this case, however, it has been the subterranean movement, and not the glacier, which has determined the basinlike character of the depression.

It is impossible to separate the question of the origin and causes of valley depressions on land, from the origin and causes of the greater depressions which are occupied by the ocean, or the question of the origin of mountains on the land from that of the origin of mountains rising out of the bed of the sea. The sea itself has no denuding power except within a very limited distance of its own mean level, and except when powerful and shifting currents may alter the arrangement of the material which it is itself depositing; and subterranean movement must in both cases be the great agent in determining both these forms of action. It is impossible, I think, to look at the striking scenery of the Hebrides without seeing that many of the island groups represent the remaining fragments of an extensive country which has been first entirely covered by volcanic eruption of enormous extent in space, and of immense duration in time, and secondly, by subsequent upheavals and depres-

sions, has been so broken up that it is impossible to have a guess as to its former levels or as to its former surface. If it be true that the Scoor of Eigg is the remains of a lava current which filled some great river-channel in Oolitic rocks, it is evident that the land in which and from which that river ran is now completely broken up, and the fragments which remain are for the most part buried deeply under the masses of igneous rock which were poured out by volcanic vents. The discovery, which I was so fortunate as to make many years ago, of Miocene vegetation, preserved under the traps and ash-beds of Mull, proves that these volcanic vents were in active operation at least down to that Tertiary age. It is equally evident from the precipitous fractures which the remaining sheets of Trap present in Mull, almost everywhere along its shores, but especially on its southern coast, as well as from the position of the fragments remaining in the sea, of which Staffa is one, and the surrounding islets are more, that the whole region has subsequently been again the scene of immense elevations and subsidences. The remarkable configuration of the bottom of the sea on the south coast of Mull indicates comparatively recent operations of this kind, by the presence of submarine heights and hollows which have not yet been filled up by deposited material.

Here again, however, I would observe that there can be no doubt of the extent of material which has been removed. Not one of the volcanic vents, which must have existed in abundance, has survived in a form which can lead with any certainty to their identification. The presence of very slag-like material on one shoulder of Ben More leads me to believe that this fine mountain is the remaining core of what was once a volcanic vent. Most of the loose material, however, has been removed; but as regards the agency of the glacial epoch in that removal, I believe it to have been comparatively trifling. The evidences of the work of that epoch, however, are abundant. Those marks indicate, so far as I have seen them, that the general "lie of the land" and the general configuration of the surface pre-existed substantially as they are now, and guided and controlled all the glacial action. The island of Iona, for example, in which there is not a scrap of granite rock *in situ*, is literally strewn with blocks from the opposite low hills upon the Ross of Mull; and there is one immense boulder much rounded and worn, the original position of which in the parent rock can be identified with almost absolute certainty from the occurrence in it of a peculiar vein or string of uncombined material, which marks the granite at a par-

ticular spot on the coast of Ross. This and other circumstances determine the direction of the movement of the floating ice-floes or bergs which brought the granite. That direction points straight to the high group of hills, or rather mountains, which lie to the east, which encircle Loch Scriddan on both sides, and one of which is the lofty summit of Ben More. If there was gathering-ground for a great glacier anywhere in that region, it must have been upon Ben More especially. And, on the other hand, the movement of an ice-sheet from the north-east or from the north, could not have carried a single fragment of granite to the island of Iona. I observe that in the interesting paper by Mr. Allport on the Igneous Rocks of Arran (in 'Geol. Mag.' vol. ix. p. 537), he remarks upon the fact that in the district he examined he did not see a single boulder which did not belong to the island itself. In like manner, Mr. Tiddeman tells us that the transported blocks in North Lancashire all belong to the drainage-system in which they are found, except those on the western margin of the district, where that drainage-system opens out upon the plains, and the country is of course exposed to transporting agencies coming from a different group of hills. And here I may observe that whilst the absence of boulders foreign to any district is very strong evidence against an ice-sheet coming from beyond that district, the converse does not always hold true. The presence of boulders not belonging to a district may be no proof, or even indication, of an ice-sheet. If a district is not sheltered from drifting ice coming from a distance, the presence of foreign boulders affords no proof or presumption of the action of an ice-sheet such as has been supposed, because such boulders may have been brought by floating ice. Thus, for example, the presence upon the coast of Antrim of boulders or blocks of mica-slate belonging to the Mull of Kintyre would be no indication of an ice-sheet filling up the Irish Channel, and moving up the hills of the opposing coast—because if there is one point on the whole coast of Scotland where icebergs would be sure to break off and float away, it must have been on the Mull of Kintyre, the termination of a mass of hills of considerable elevation and ending in precipitous faces of very highly fractured and contorted beds of mica-schist. Among the interior hills and valleys of Argyllshire, covered as they are with transported boulders and perched blocks, I have never observed any belonging to rocks which do not exist somewhere in the drainage-system within which they occur.

In reasoning on the effects of the glacial epoch upon the configuration of the surface, I cannot admit that we are at liberty, far less bound, to follow my distinguished kinsman, the author of 'Frost and Fire,' in the endeavour which he says* he has made to "shake off ideas of size." Size (that is to say, the magnitude of moving bodies) is an essential element in their working power, and must have inseparable relation to the magnitude of their possible effects. Nor are we at liberty to magnify any known agency to some enormous imaginary extent in order to make it adequate in conception to the effects which our theory requires us to ascribe to it. If the relation between possible weight and possible work is to be got rid of, there can be no limit to our fancies in respect of the powers of ice. If we ascribe to it the formation of all our existing valley-systems—if our hills and mountains are nothing but the knobs and protuberances which some ice-sheet of gigantic depth, and moving with gigantic force, has left standing out of a Plane of Abrasion, I see no reason why we should stop short of supposing that the bed of the Atlantic has been dug out by the same agency. Already I observe that the soundings of the 'Challenger' have added another deep rock-basin outside the Straits of Gibraltar, in continuation of the two already known in the Mediterranean, separated from each other by submarine hills of considerable elevation. If the rock-basins above water have all been hollowed out by ice, why not also the rock-basins below the sea? If, on the other hand, these great submarine depressions are generally recognized as due to subterranean subsidences, why not admit the probability of lesser depressions at a higher level being due to the same great cause?

The question on which we are now speculating may be put thus: what was probably the character of the northern hemisphere (the physical geography of the country) before the commencement of the glacial epoch, as compared with what it now is? But in speculating on this question we must remember that even if we could restore the old surface exactly, we should not necessarily know how much or how little of the change has been the effect of ice, because it is at least possible, nay, very probable, that the coming on of glacial conditions may have arisen from great terrestrial changes due to upheavals and depressions of the crust. And even if this was not the case, even if the coming-on of glacial conditions was due to causes

* "On the Glaciation of Ireland," by J. F. Campbell, Quart. Journ. Geol. Soc. vol. xxix.

purely cosmic and not terrestrial, it seems almost certain that the glacial epoch lasted through and during immense movements of elevation and depression. These may have been more or less sudden or more or less slow. But my own belief is that to these, and not to glacial action, have been always due the great determining lines of physical geography. To them, of course always accompanied and followed up by marine denudation, has been due the almost complete destruction of the lands and continents which, before the glacial epoch, supported an abundant and splendid Miocene vegetation—lands and continents which, we know, from the scattered fragments it has left, extended from our own temperate region to very near the Pole. When we contemplate the changes of surface, as enormous as they are mysterious, which are indicated by those proofs of a Miocene vegetation having flourished so lately in the geological history of the globe in regions which are now glacial, and when we recollect that these changes must have been accomplished long before what we call the glacial epoch began, it does not seem reasonable to ascribe to so comparatively recent and to so comparatively superficial a cause effects which have been a thousand times repeated by agencies more permanent, more deeply seated, and more constant in their operation.

It is not without misgiving that I venture to express so strong an opinion against the belief of some of the most distinguished geologists now living. But I recollect, with some satisfaction, that the history of geology, like the history of other sciences, is the history of the prevalence of particular theories at particular times—not generally to be wholly abandoned, but almost always to be greatly modified. I have a strong impression that the glacial theory is now at about its maximum, and that when all our valley-systems are described as being nothing but magnified striæ*, we are pretty near the summit-level of this particular excursion of the scientific imagination. At all events I have the satisfaction of remembering that if this glacial theory be true, it can be but more firmly established by the statement of objections which are capable of being removed, and of difficulties which are capable of being solved.

* "Hair-lines, Irish glens, and Norwegian fjords are all grooves of one pattern engraved on different scales" (J. F. Campbell, on the Glaciation of Ireland).

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